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CONDUCTED BY

SIR DAVID BREWSTER, K.H. LL.D. F.R.S. L. & E. &c.

RICHARD TAYLOR, F.S.A. L.S. G.S. Astr. S. &c.

AND

RICHARD PHILLIPS, F.R.S. L. & E. F.G.S. &c.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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ALERE FLAMMAM

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ERRATA.

Page 156, line 25, *for* coming *read* common.
—— 157, — 27, *for* Plate I. *read* Plate II.
—— 165, — 2, *for* chlorate of potash *read* chlorate of lime.
—— 167, — 4, *for* 31·2910 *read* 81·2910.

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[THIRD SERIES.]

JULY 1832.

I. *On the Refraction of the differently-coloured Rays in Crystals with one and two Axes of Double Refraction.* By M. Rudberg*.

As the remarkable discovery of Fraunhofer of the dark rays in the spectrum gives an unexpected degree of precision to researches on the refraction of coloured light, it becomes interesting to determine by this accurate method the indices of refraction. With the view principally of constructing more perfect achromatic object-glasses, Fraunhofer himself determined the refraction of the differently-coloured rays for several kinds of flint and crown glass, and also for some other substances that have simple refraction†. But for doubly refracting crystals similar researches are entirely wanting, which might show in general how the double refraction varies with different colours, and how this variation produces the different inclination of the optic axes which Mr. Herschel has observed for the differently-coloured rays in crystals with two axes. Besides, an inquiry into the double refraction of coloured light would add to the small number of accurate determinations of dispersion which we owe to Fraunhofer.

In order to measure accurately the deviation of the ray refracted by a prism, as well as its own refracting angle, I used a repeating circle constructed by Lenoir, which Swanberg had used in the measurement of the degree of the meridian in Lapland, and which being divided centesimally gave im-

* From the *Annales de Chimie et de Physique*, tom. xlviii. p. 225.

† See Phil. Mag. and Annals, N.S. vol. ii. p. 401.—Edit.

mediately by the mean of the verniers of the alidade 50″ centesimal, or nearly 16″ sexagesimal. The instrument was arranged in the following manner. The limb being placed horizontally, the upper telescope attached to the alidade was taken away and placed upon one of the arms of a lever, the middle of which rested on the centre of the limb, and the other arm of which was loaded by a counter weight equal to the weight of the telescope. The whole was combined in such a manner that in turning the alidade, we turned the telescope, the object-glass of which described, in consequence, an arc of a circle round the centre of the instrument. To this centre was applied a rod of copper, forming a continuation of the axis. This rod carried a plate about four inches in diameter, above which there was fixed, by means of six screws, at the distance of some lines, another plate, which could by this means be made horizontal. In a hollow of this was a ring of copper, which carrying a plate of ground glass, and having its circumference toothed, turned by means of a screw, so that the prism which was always placed on the plate of glass having its edge in the centre, could be brought into such a position that the refracted ray had its deviation a minimum.

The sun's light was introduced into the dark chamber through a small opening by means of a heliostate at the distance of 33 feet from the centre of the repeating circle. The opening in the window-shutter formed by two plates, one of which was moveable by a screw, could be rendered more or less narrow.

The limb remained immoveable during the observations. To be sure of this, the other telescope, which was on the lower surface of the limb, had its cross wires directed to an object which was situated on the other side of the Lake Malarn, at a distance of more than 2500 feet.

In order to measure the refracting angle of the prism, the refracting edge being in the centre of the circle, the prism was turned in such a manner that there could be seen successively, by means of the telescope, the two images of a mark which was reflected from the two faces whose intersection formed the edge of the prism. The mark was the bar of the window of a house on the opposite bank of the Lake Malarn, at the distance of more than 2500 feet. It is evident that by turning the point of intersection of the crossed wires respectively, upon each of the images of the mark reflected from the two faces, the angle which the telescope describes is exactly double that of the prism.

With respect to the angle of deviation, the prism was turned in such a manner that the angle of deviation of the refracted

ray was at first the least possible, for example, to the left, and then the least to the right, so that the angle described by the telescope was always exactly the double of the angle of deviation.

The black lines of which I measured the deviation were those marked B, C, D, E, F, G, H by Fraunhofer. I did not put the prism into the position of least deviation for each individual ray, but put it into this position for one. I measured the double deviation for this one and the others, leaving the prism immoveable, which renders the observation more easy and more accurate.

The indices of refraction are easily calculated by this method of operating. The angle which the incident ray makes with the anterior face of the prism being $= 90° - x$, the angle of the refracted ray with the same face $= 90° - z$, the deviation $= \Delta$, the angle of the prism $= \varepsilon$, and the index of refraction $= n$, we have

$$\sin x = n \,.\, \sin z$$

$$\sin (\Delta + \varepsilon - x) = n \,.\, \sin (\varepsilon - z).$$

If the prism is at the minimum of deviation for a certain ray, we shall have for it $x = \frac{1}{2} (\Delta + \varepsilon)$ and $z = \frac{1}{2} \varepsilon$. Hence

$$n = \frac{\sin \frac{1}{2} \Delta + \varepsilon}{\sin \frac{1}{2} \varepsilon}.$$

For another ray whose deviation in the same situation of the prism is $= \Delta - \delta$, and whose index of refraction is $= n'$, we shall have

$$\sin \tfrac{1}{2} (\Delta + \varepsilon) = n' \sin z'$$

$$\sin \left(\tfrac{1}{2} (\Delta + \varepsilon) - \delta\right) = n' \sin (\varepsilon - z').$$

and making $z' = \frac{1}{2} \varepsilon + \zeta$, we obtain

$$\sin \tfrac{1}{2} (\Delta + \varepsilon - \delta) \,.\, \cos \tfrac{1}{2} \delta = n' \sin \tfrac{1}{2} \varepsilon \,.\, \cos \zeta.$$

$$\cos \tfrac{1}{2} (\Delta + \varepsilon - \delta) \,.\, \sin \tfrac{1}{2} \delta = n' \cos \tfrac{1}{2} \varepsilon \,.\, \sin \zeta.$$

And consequently,

$$\text{tang}\, \zeta = \text{tang}\, \tfrac{1}{2} \delta \,.\, \text{tang}\, \tfrac{1}{2} \varepsilon \,.\, \cot \tfrac{1}{2} (\Delta + \varepsilon - \delta).$$

Having calculated by this formula the value of the angle ζ, we obtain

$$n' = \frac{\sin \frac{1}{2} (\Delta + \varepsilon)}{\sin (\frac{1}{2} \varepsilon + \zeta)}.$$

Section 1.—*Refraction in Crystals with one Optical Axis.*

1. *Rock Crystal.*—Two prisms of the crystal were cut in such a manner that the edge of the prism was parallel to the axis of crystallization, and consequently the two rays followed the law of simple refraction. The prism was put into such a po-

sition that the line H in the extraordinary spectrum was reduced to its minimum of deviation; and the other lines not only of this spectrum, but of the ordinary spectrum, were all measured in the same position of the prism.

As the two spectra always cover one another in part in rock crystal, I employed, in order to be able to observe each of them separately, a plate of tourmaline cut parallel to the axis. When put before the aperture of the eye-glass, it allowed only the light of the extraordinary spectrum to pass when its axis was parallel to the axis of crystallization; and when its axis was in a position perpendicular to this, it allowed only the ordinary light to pass.

The angle of one of the prisms was $52^{\circ}{\cdot}940$ or $47^{\circ}\ 38'\ 46''$, and that of the other $50^{\circ}{\cdot}372$ or $45^{\circ}\ 20'\ 5''$. These values are the means of several observations, which giving immediately the double angle do not differ more than $0^{\circ}{\cdot}005$ in $16''$. The observations from which the following indices were calculated, were made at a temperature of $+18^{\circ}$ centigrade.

	Extraordinary Spectrum.				*Ordinary Spectrum.*		
Fixed lines.	Prism No. 1.	Prism No. 2.	Diff.		Prism No. 1.	Prism No. 2.	Diff.
H	1·56769	1·56776	0·00007	...	1.55814	1·55821	0·00007
G	1·56361	1·56369	0·00008	...	1·55421	1·55429	0·00008
F	1·55892	1·55896	0·00004	...	1·54960	1·54970	0·00010
E	1·55629	1·55634	0·00005	...	1·54709	1·54714	0·00005
D	1·55325	1·55331	0·00006	...	1·54414	1·54423	0·00009
C	1·55083	1·55088	0·00005	...	1·54179	1·54184	0·00005
B	1·54987	1·54994	0·00007	...	1·54088	1·54093	0·00005

The differences are evidently only errors of observation.

By taking the mean of the two results, we shall have the following indices, which can scarcely err 0·00005.

Lines.	Extr. Ray.	Ord. Ray.
H	1·56772	1·55817
G	1·56365	1·55425
F	1·55894	1·54965
E	1·55631	1·54711
D	1·55328	1·54418
C	1·55085	1·54180
B	1·54990	1·54090

Malus had found for the extraordinary ray the index 1·55817, and for the ordinary ray 1·54843 *, which being both situated between E and F, correspond as well as could be expected; as in his time the fixed points or lines in the spectrum were not known.

* Malus found these two numbers to be the two indices, but he considered 1·54843 as the *extraordinary index*. It was M. Biot who discovered that the larger index was the extraordinary one.—Edit.

With regard to the question of the dispersion of the two rays in rock crystal, we find by comparing the ordinary and the extraordinary indices for the different colours, that the double refraction is greater for the violet light, and less for the red light; or in general, *that the double refraction is as much stronger, as the individual refrangibility of the ray itself is greater;* for by calling n' the index of the ordinary ray, and n'' that of the extraordinary ray, we shall have the following values of $\frac{n''}{n'}$ for the different colours:

H	1·00613
G	1·00605
F	1·00599
E	1·00594
D	1·00589
C	1·00586
B	1·00584

Whence it follows that the ratio $\frac{n''}{n'}$ goes on increasing from the red to the violet extremity of the spectrum, and consequently that the difference of the velocities of the two rays increases for the different colours in the same direction.

2. *Calcareous Spar.*—From this crystal I caused to be cut two prisms having their edge parallel to the axis of crystallization. I could use only one of them, the angle of which was 66°·577, or 59° 55′ 9″.

In the extraordinary spectrum it was the line H which was reduced to a minimum of deviation, and the prism remained in this position during the measurement of the other lines of the spectrum. In the ordinary spectrum, on the contrary, which had an extent three times greater, the violet light was very feeble, and the line H very wide; and I chose the line F, which was reduced to the least deviation. Since the prism remained in this position, it is evident that in the value of the index n', we must make δ negative for the two lines H and G, but positive for the lines E, D, C, and B. The mean indices are given in the following table. The temperature was $+17°\frac{3}{4}$ centigrade.

	Ord. Ray.	Extr. Ray.
H	1·68330	1·49780
G	1·67617	1·49453
F	1·66802	1·49075
E	1·66360	1·48868
D	1·65850	1·48635
C	1·65452	1·48455
B	1·65308	1·48391

The indices given by Malus are 1·6543 and 1·4833. Calling, as before, n' the index of the ordinary ray, and n'' that of the extraordinary ray, we have the following values of the ratio $\frac{n'}{n''}$.

H	1·12385
G	1·12154
F	1·11891
E	1·11750
D	1·11582
C	1·11440
B	1·11400

From which we see incontestably *the increase of the double refraction with the individual refrangibility of the colour.*

[To be continued.]

II. *Observations on the preceding Memoir. By* Sir David Brewster, *K.H. LL.D. F.R.S. V.P.R.S. Ed.*

It is impossible to estimate too highly the value of the observations contained in the preceding memoir. To the mineralogist, as well as to the optical philosopher, such observations are fixed data of the highest utility.

The only general conclusion, however, which M. Rudberg has drawn from his experiments in the first part of his memoir, is, *that the double refraction increases with the refrangibility of the coloured ray;* or, to express the same fact in other words, that the dispersion produced by the extraordinary refraction is greater in proportion to the mean refraction than that produced by the ordinary refraction,—or that doubly refracting crystals have two different dispersive powers.

This discovery was made by myself so long ago as 1812, and an account of it published in my *Treatise on New Philosophical Instruments*, Edinb. 1813, p. 312–315: in the Philosophical Transactions, 1813, p. 107; and in the article *Optics*, in the Edinburgh Encyclopædia, vol. xv. p. 544. The following is the account of it, which I have published in the first of these works.

"The most singular result, however, which is contained in the following table (Table of Dispersive Powers) relates to the dispersive powers of doubly refracting substances. The first experiment which I made upon crystals, was to determine the dispersive power of Iceland spar; and from a cause merely accidental, I corrected the colour of the least refraction. The result thus obtained was 0·026, considerably below water,

which stands at 0·035 of the scale; and upon comparing it with the place assigned to Iceland crystal by Dr. Wollaston, I was surprised to find that he placed its dispersive power very considerably above water, and even above diamond. This unexpected difference between the two measures induced me to repeat the experiments, not only with other prisms of the Iceland spar, but also with other standard prisms of flint and crown glass. These new results served only to confirm the accuracy of the first experiment, and to strengthen my suspicion that Dr. Wollaston had committed a mistake. As this reasoning, however, was founded on the assumption which both Dr. Wollaston and I had made,—that the spar had only one dispersive power,—I resolved to measure the dispersive power of the extraordinary refraction. This new value having turned out to be greater than that of water, I immediately saw that Dr. Wollaston had measured the colour of the greatest refraction, while I had measured the colour of the least; and that this remarkable mineral, which had so long perplexed philosophers by its double refraction, possessed the no less extraordinary and inexplicable property of two dispersive powers. In subjecting to examination other crystals that afforded double images,—such as carbonate of strontites, carbonate of lead, and chromate of lead,—I found that every separate refraction possessed a separate dispersive power. This general law, though not repugnant to any optical phænomena, is still of such a nature, that it could not have been inferred *à priori* from any relation which is known to subsist between the refractive and dispersive powers. No person, indeed, has even conjectured that a double dispersive should accompany a double refractive power: and if we were to reason in this case from an analogy founded on experiment,—an analogy, too, which is by no means remote, we should certainly conclude, contrary to the fact, that the greatest refractive power would be accompanied with the least power of dispersion. In all the minerals in which a metal is the principal ingredient, those which have the greatest refractive density have also the greatest faculty of producing colour; while in all the precious stones a high refractive power is attended with a low power of dispersion. This remarkable property of a double dispersion, therefore, is contrary to the general results indicated by experiments; and though it appears to exclude some of the theories by which a double refraction has been explained, it certainly adds another to those numerous difficulties with which philosophy has yet to struggle, before she can reduce to a satisfactory

generalization the anomalous and capricious phænomena which light exhibits in its passage through transparent bodies."

In comparing the results obtained by M. Rudberg with calcareous spar with those obtained by Malus, we have been struck with a discrepancy so great that we cannot find any explanation of it.

Malus found the two indices for quartz to be 1·55817 and 1·54843. Now if we compare these numbers with those of M. Rudberg, we shall find that they both correspond to a ray towards the extremity of the *green* space between the lines E and F. Thus

		Extraor. Ray.	Diff.	Ordin. Ray.	Diff.
Rudberg...	F	1·55894	77	1·54965	122
Malus		1·55817	186	1·54843	132
	E	1·55631		1·54711	

Hence Malus's extraordinary index corresponds to a ray whose distance from F is 77; while his ordinary index corresponds to a ray whose distance from F is 122. This discrepancy is not at all to be wondered at, and shows us how uncertain are all measures of refractive powers unless they are referred to the fixed lines in the spectrum.

The case is widely different, however, with calcareous spar, as the following table shows.

		Ord. Index.	Diff.		Extr. Index.	Diff.
Rudberg...	C	1·65452	23	B	1·48391	61
Malus......		1·65429	121		1·48330	
Rudberg...	B	1·65308				

Here the results of Malus and Rudberg are widely divergent. Malus used the middle green ray of the spectrum (*Théorie de la Double Refraction*, Deux. Parte, § 42), or that which is half way between E and F, and yet his mean ordinary index corresponds with one of the red rays in Rudberg's observations! Assuming the same ray in both observations, the mean ordinary index will be as follows:

	Mean Ord. Index.	Diff.
Rudberg......	1·66585	1156
Malus	1·65429	

In the case of the extraordinary ray the discrepancy is still more surprising. Here the mean index of Malus corresponds to a ray distant 61 from B, and existing *beyond the end of the visible spectrum formed by light of ordinary intensity.* Assuming

the same ray in both observations as the mean one, the mean extraordinary index will be as follows:

	Mean Extr. Index.	Diff.
Rudberg..................	1·48971	641
Malus....................	1·48330	

Dr. Wollaston gives the two indices according to his measurements, as 1·657 and 1·488, which are nearer those of M. Rudberg; but as Dr. Young has assured us that the measures taken by Dr. Wollaston are *appropriate to the extreme red rays*, they throw no light upon the cause of the discrepancy under our consideration. The discrepancies, indeed, become more alarming, and will stand thus, by calling B the extreme red ray, and calculating its index from Malus's index for the middle green.

	Red Ray B.	
	Ordinary.	Extraordinary.
Wollaston ...	1·65700	1·48800
Rudberg......	1·65308	1·48391
Malus	1·64152	1·47750

Hence it appears that Wollaston's measures are greater than those of Rudberg, and still more remote from those of Malus.

As it is impossible to suppose for a moment that either Malus or Rudberg could have committed errors of observation capable of reconciling these results, we are forced to conclude, that the experiments of each were made with crystals of calcareous spar which had different degrees of refractive power,—a conclusion which deprives us of the hope of obtaining invariable physical data for different minerals. Such a result, indeed, might have been expected from the variety of specific gravities which different specimens of calcareous spar exhibit; and all that science can hope to accomplish on this subject, is to define the general limits within which these variations are confined.

III. *Observations respecting the Invention of the Telescope. By* J. E. Drinkwater, *Esq.**

A VERY interesting article on the invention of telescopes is printed in the second and third Numbers of the Journal of the Royal Institution, in which it is clearly proved that John Lippershey, a spectacle-maker of Middleburg, possessed the invention on the 2nd of October, 1608: that Jacob Adri-

* Communicated by the Author:—In the first series of Phil. Mag. vol. xviii. p. 245, and vol. xix. p. 66, will be found two letters, with extracts from old English books, from which the writer infers that the telescope was known in England long before the period of its reputed invention.—Edit.

aansz (commonly called Metius) also possessed the art of making them on the 17th of the same month, and that he then professed to have been engaged in the experiments which led him to it during two years previously; and it is advanced as probable that Hans, or his son Zachary Jansen, invented a compound microscope about 1590.

Dr. Moll, by whom this paper was communicated, has done me the honour to notice my sketch of the Life of Galileo, in which I had arrived nearly at the same results with respect to the claims of Lippershey and Jansen; although some of my statements were necessarily imperfect, from want of access to those official records, now for the first time produced from the Dutch archives. Dr. Moll has, however, thought fit to comment on some assertions of mine in terms which call for some reply on my part: this would have been attempted earlier, had I earlier seen the paper in question.

Dr. Moll has also pointed out an error committed by me in calling both Peter Borel the author of the treatise *De vero Telescopii Inventore*, and William Boreel the ambassador, by the Italian name Borelli; and a similar error in translating the Latin name of Van den Hore, whom I have confounded with his contemporary Gärtner, both using the same Latin signature Hortensius. For these, and I fear many other errors as well as omissions in that essay, I have little apology to offer, and feel nothing but obligation to those who may be at the pains to discover them. But I wish to defend myself (even when writing anonymously) from the charge which Dr. Moll insinuates, of affecting to quote from books which I know only by extracts. I protest against this practice as a dishonest one, by which stories often obtain currency and credit on the supposition that they have been examined by several authors, who in fact have merely copied one from another. I consider it essential to the truth of history that the *real* authority should be cited whenever any is given. In the only instance in my own case where I was not writing either with the original authority before me, or an extract copied out of it with my own hand, I have given a double reference (p. 58.) to the author whose statement I repeated, and to the manuscript from which he professed to have drawn his account. It may perhaps be true that Borel's book is scarce; but I found it in the British Museum, which is tolerably rich in scientific works of the sixteenth and seventeenth centuries; indeed the copy which I used there seems more perfect than the one alluded to in a note on Dr. Moll's article, for it contains a portrait of Jansen as well as of Lippershey.

Although my principal object in this communication is to

vindicate the honesty of my reference, I cannot refrain from attempting also some answer to Dr. Moll's challenge "to point out the passage in Borel's book, in which either Boreel or John the optician exhibit the least intention of throwing Galileo's discoveries into the shade." The charge I made was against the author; I have no quarrel with the ambassador, unless so far as he might be concerned in getting up the book in question; and I have Dr. Moll's own authority for stating that it was written "probably at his request, and certainly with his assistance". It is not indeed easy to convey the same impression by detached extracts, which is produced by the tone of the whole volume; but a few passages may be especially noticed. The eighth chapter contains the following remarks: "Some contend that it (the telescope) was known to Bacon the Englishman: some to Baptista Porta, who seems to have said something on the subject, but obscurely.—But the opinion of the majority has been in favour of Drebbel, Galileo, and Metius; *and they themselves do not blush to claim it*, although it may be made clear to every one by public documents that they had recourse to an artist of Middleburg, or borrowed it from him in some way." This charge is repeated with more particularity against Galileo in the next chapter: "Among this crowd of inventors first appears Galileo, *who attributed the invention to himself*, and to this hour has been puffed as the real inventor, and has exalted himself by his own praises, as appears by his petition to the States of the Republic of Holland." Borel chooses to disprove this supposed claim by quoting the Mercurio of Vittorio Siri, who relates how Galileo rediscovered the instrument on hearing that such a thing had been done in Holland. A more candid writer would have referred to one of Galileo's own statements, such as that in the Saggiatore, where he mentions "l' Ollandese primo inventore del telescopio", or he might have given precisely the same account which he has adopted from Siri, out of Galileo's *Nuncius Sydereus.* He there says, "A rumour reached me about ten months ago, that a perspective had been worked by a Belgian, by help of which objects, though at a great distance from the eye of the observer, appear as distinctly as if near: and of this certainly admirable contrivance some experiments were noised about, which some believed, others denied. The same thing after a few days was confirmed to me in a letter from Paris by James Badovere, a French gentleman, which at length occasioned that I set myself intently to examine the reason of the thing, and to contrive the means of inventing a similar instrument, in which I soon succeeded by help of the theory of refractions," &c. Borel had this passage before him; for he has incautiously printed it in another part of his

book, where he is discussing the subsequent progress of the invention; and it is not easy to imagine why it was suppressed in this place, except that it would necessarily have interfered with his plan of mentioning Galileo as one who shamelessly endeavoured to rob the first inventor of the credit due to him. In his petition to the States, nothing whatever is said about the invention of the telescope. This first instance of Borel's unfairness made me examine the rest of the book with more jealousy. In the twelfth chapter we are told that Zachary Jansen discovered the telescope in 1590, and "*immediately* applied himself to the discovery of stars and other novelties".—"This new Dædalus saw more with one tube and a single eye than did Argus *or Lynceus*."—"*In the moon too, he was the first to discover spots; and afterwards Galileo following his example observed the same more accurately*." These passages (in which the allusion to the society of *Lincei*, of which Galileo was a member, must not be overlooked,) seem to me to justify the first part of my assertion, that Borel "endeavoured to secure for Jansen and his son the more solid reputation of having anticipated Galileo in the useful employment of the invention." No one had heard of these pretended observations till Borel published his book in 1655, thirteen years after Galileo's death; nor do they rest on any proof but Borel's own declaration. There is indeed a communication from Jansen's son John, with respect to his own discoveries, but it does not contain a syllable in support of his father's. The substance of this communication is given in the fourteenth chapter, which Borel entitles, "The excellent evidence of the above-named inventors, by which the foregoing statements are supported." Dr. Moll finds fault with me for calling this statement a letter from John, and in fact it appears that it is only compiled from such a letter. I was misled by Borel himself, who, in referring to it, invites his readers to "learn what John himself has communicated by *his own letters*, though there are no means of confirming his statement by other evidence." In this occurs the following passage, amongst accounts of other discoveries which John positively claims as his own. "I have often observed the planet Jupiter, which appears round and indistinctly spherical. Near him I have occasionally found two little stars, situated at or near the upper part; sometimes also three. But generally I have seen four; and as far as I have been able to observe, they circulate continually round Jupiter: but this I leave to astronomers." This is the passage by which it seems to me that Borel intended to hint away (as far as he durst) Galileo's claim to the earliest discovery of Jupiter's satellites; and it is remarkable that this communication is given entirely without date, in a work which, being written to establish a chronolo-

gical fact, is everywhere else very particular in that respect. It certainly is not, as Dr. Moll would have us believe, a mere optician's report of the performance of his telescopes, but is "the excellent evidence", by means of which the discoveries of John and his father, "redounding so much to the credit of themselves and their country", were to be supported. What is the meaning of that remarkable expression, "there are no means of confirming his statement by other evidence"? What was it that required additional proof? As Dr. Moll most truly observes, "Thousands, certainly hundreds, saw the satellites in 1655; and why should not John, like other people?" That which struck me as remarkable was not that John should have seen them whenever he wrote, or was supposed to write, that letter, but that in 1655 Borel should think worth while to insert amongst his "excellent evidence" a declaration that he had; accompanying it with the cautious remark, that he had no proof of it beyond John's own assertion. This observation acquires additional force from the correction, for which I am indebted to Dr. Moll, that John's whole letter is not given; and therefore it is to be presumed that Borel extracted from it only that which he thought important. Surely the mere fact that Jupiter's satellites were visible in his glasses did not merit that distinction in 1655: if they were not, his trade would scarcely have found him a livelihood. If he wanted to give a proof *how much* his glasses were capable of showing, it would have been more decisive of their excellence to declare whether or not he had ever seen Saturn's satellite, then recently discovered by Huyghens, of which discovery Borel gives an account in this same book. Even in mentioning the satellites, he would have said simply, "I have seen Jupiter's satellites", and would not have given all the particulars of seeing sometimes four, sometimes two, or three, &c., unless he was speaking, or wished to be understood as speaking, of something of which he had never before heard. Mr. Dollond or Mr. Tully (if I may be allowed to borrow Dr. Moll's own illustration), should they be asked at the present day for an account of their best glasses, would scarcely think of stating that there is something like a ring round Saturn; and that, so far as they can judge, it appears to revolve about the planet: nor, if they should communicate such information to Dr. Moll, would it occur to him to quote it as "excellent evidence" of the discoveries of these gentlemen. It was not more to the purpose in 1655 to print John's remark, that, so far as he could tell, there were four satellites which appeared to circulate round Jupiter; since the fact that they did so had been indisputably established more than forty years, their periods had been calculated, and their future appearances predicted;—unless indeed there was a concealed intention of suggesting the idea at some future period, that John

had observed these stars independently of Galileo, and if independently, perhaps anteriorly.

If it be thought that I have put a meaning on this passage which it was not intended to bear, some excuse may still be found in the fact, that whether or not Borel intended to lay the foundation of a future claim, this end, which as I contend he had in view, has actually been attained. In the Encyclopædia Britannica, under the article 'Optics,' the following remark occurs, after the substance of Borel's account has been stated: "*This, it is probable, was the first observation of Jupiter's satellites*, though the person who made it was not aware of the importance of his discovery." In Dr. Young's Lectures on Natural Philosophy, the same idea has resulted from the perusal of this passage. Dr. Young says: "The first person, who is certainly known to have made a telescope, is Jansen, a Dutchman; —*and one of his family discovered a satellite of Jupiter with them.* Galileo had heard of the instrument, but had not been informed of the particulars of its construction: he reinvented it in 1609, and the following year *rediscovered also the satellite which Jansen had seen a little before.*" It cannot therefore be doubted that owing to the manner in which Borel has introduced this account, John, the son of Zachary Jansen, has had the credit of the discovery given to him: it cannot be denied that Borel has claimed for Jansen himself the credit of first using the telescope for celestial observations without producing any proof of his assertion, and that he has spoken of Galileo in unbecoming terms, and has represented him, contrary to truth, and in the face of his own declaration, as denying the source whence he derived his first knowledge of the instrument. Finding the error which I have just mentioned in works of such reputation, and thinking myself also that Borel's account was artfully prepared with a view to produce that very misconception, I thought worth while to observe that John was only six years old in 1609, when the satellites were discovered by Galileo, and that therefore his claim must be put out of consideration. As to the question of Borel's intentions, on which my opinion remains unaltered, I am not so anxious to bring others to agree with me, as to show that I did not venture a random contradiction of a previous statement, without examination of the point on which I pretended to give an opinion.

I have one remark only to make on another question connected with this subject: Dr. Moll is unwilling to believe that in 1637 the Dutch were inferior to the Italian telescopes, as I asserted on the authority of Hortensius, who wrote to Galileo that none could be procured in Holland sufficiently good to show Jupiter's disc well defined, and of Gassendi, who wrote to him that he could not procure a good one in Venice, Paris, or Amsterdam. Dr. Moll does not notice Gassendi's

remark, but says that "Hortensius wanted more than could be accomplished in his time; and even now telescopes *of a certain size*, which show Jupiter's disc well defined, are not of every day's occurrence." The term 'well defined' will of course bear different meanings in different stages of the art: and it is probable that Dr. Moll would be dissatisfied with the performance of a glass, with which Huyghens would have been enraptured. Such expressions are necessarily comparative, and we can only attach a 'well defined' meaning to them by comparing contemporary statements. The letter of Hortensius to which I alluded contains the following passage (*Opere di Gal.* tom. ii. p. 466. Ed. Pad.): "De telescopio agere cæpimus, comperimusque nulla in Bataviâ hodie quæ tantam præcisionem polliceri queant, quanta ad eas observationes requiritur. *Solent enim etiam optimi discum Jovis hirsutum offerre et malè terminatum*, unde Joviales in ejus vicinia non recte conspiciuntur.—Omnes artifices rudes experimur, et Dioptricæ quam maximè ignaros." Galileo answered in the folllowing terms (p. 474): "Quanto al secondo punto che è del trovarsi Telescopij di maggior efficacia di quelli che si fabbricano costì; mi pare d' aver scritto altra volta la facoltà di quello che ho adoprato io esser tale, che *mostra primieramente il disco di Giove non irsuto ma terminatissimo, non meno che l' occhio libero scorga il lembo della Luna*, e cosi terminati mostra ancora i Satelliti di quello." And in a subsequent letter to Deodati he says again (p. 472): "Mi vengono ancor domandati dell' istesso Sig. Ortensio i vetri per un Telescopio, i quali sieno di perfezione tale che mostrino ben terminato il disco di Giove, e chiaramente apparenti i quattro suoi Satelliti, effetto che, come egli scrive non se ha da quelli, che si fabbricano in Olanda: se me succederà prontamente il farne provisione, gli invierò a V. S. molt. Ill. insieme colle presente." C. Huyghens had made the same complaint of the inferiority of the Dutch glasses (p. 490): "Del resto i Telescopi che si fanno in queste parti non assicurandoci i quattro Satelliti di Giove, de' quali si tratta se non con certe scintillazioni le quale potrebbero impedire l' osservazione subite," &c. There is not even the pretext left that Galileo might entertain a better opinion of his glasses than others would have done; for although Gassendi was dissatisfied with the best glass he could get in Amsterdam, yet on receiving Galileo's present, he wrote (Venturi, vol. ii. p. 21): "*Eximio illo telescopio quo me beare dignatus es*, effigiari lunam procuro suis lineamentis et coloribus." These extracts show that Dr. Moll has been rather too hasty in advancing that "the assertion of the inferiority of the Dutch telescopes is wholly unsupported by proof." There are a few other trifling oversights in his valuable communication, which I forbear to notice, fearing lest my

motive in doing so should be misunderstood: nor can I conclude without expressing the great pleasure I have derived from the perusal of his paper, which has finally settled the question of the original discovery, and thrown much light on the early history of this wonderful instrument.

Athenæum, April 24, 1832. J. E. DRINKWATER.

IV. *On the Encouragement given to Science by the King of Denmark.*

IN various articles published in England relative to the decline of science, and the encouragement which is given to it by the Sovereigns of foreign countries, no notice has been taken of the King of Denmark, who has displayed an ardour and a liberality in the cause of science, in which he has not been surpassed, if he has been equalled, by any other prince.*

It is not our design at present to give any account of the scientific establishments which he so liberally supports in his own dominions, of his munificence to the men of science that adorn his reign, or of the honours which he has so judiciously conferred upon them. We propose to limit this notice to the instances of his liberality in rewarding and honouring the distinguished philosophers of other nations,—a species of patronage of the noblest and most disinterested description, and one of which there have been very few examples in the history of Europe. We trust that the example of Frederick VI. will be imitated by other Sovereigns; and that those who promote the common interests of their species by useful inventions and discoveries, will receive some acknowledgement of their services from every nation to which they have been beneficial.

The King of Denmark presented the late General Mudge, the Superintendant of the Ordnance Survey; General Muffling, the Director of the Topographical Survey of Prussia; Admiral Krusenstern, the celebrated Russian circumnavigator; Baron Alexander Humboldt; Baron Lindenau, &c. with gold chronometers, executed by the celebrated Danish artists Jurgensen and Keffels. These noble and appropriate gifts bore the simple inscription of "*Frederick the Sixth to Bernhard v. Lindenau;*" &c. The King also presented to General Fallon, the Director of the Austrian Survey, a superb pendulum clock by Jurgensen; and he sent to our own distinguished countryman, Mr. Troughton, his gold medal, with the inscription "*Merito.*"

In order to evince his high regard for foreign merit, the King of Denmark conferred the order of Dannebroga on Reichen-

* An account of the prize-medal for the discovery of a new telescopic comet, offered by the King of Denmark, will be found in Phil. Mag. and Annals, vol. xi. p. 155.

bach, Fraunhofer, Gauss, Arago, Olbers, Bessel, Encke, Struve, &c. The same order was intended for General Mudge, but it was signified to the Danish ambassador in London, that no English officer is permitted to accept of a foreign order, unless he has been employed in the military service of the state which offers it, and unless the order is given as a remuneration for his military services. This declaration prevented the King of Denmark from offering the same order, as he intended to do, to some of the most distinguished English philosophers. We are not acquainted with the military regulations here referred to; but we can assure our eminent correspondent from whom we have received these facts, that there is no power in England that can prevent a British subject from accepting of any honour that may be conferred upon him by a foreign prince.

V. *Additional Experiments on the Strength and Stiffness of Acacia. By Mr.* Peter Barlow, *Jun. As. Inst. Civ. Eng.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN my paper on the strength of different woods, printed in the Phil. Mag. & Annals for March last, I stated my regret that the results on the acacia specimens were not more satisfactory; and Mr. Bevan, in his remarks on those experiments, (Phil. Mag. and Annals for April,) having expressed the same feeling, I endeavoured to find the actual specimen alluded to in my former paper, in which the rope broke, leaving the piece uninjured, in order to repeat the experiment in a more complete manner; but I could only find a small fragment of the tree from which the former was cut. It was nearer the outside, although of the same specific gravity, viz. ·710. The largest piece I could cut from the fragment was 27 inches long and $1\frac{1}{2}$ inch square; it was supported at 25 inches. The deflection was taken very accurately, and found to amount to ·075 of an inch as each of the first four cwt. were applied: here the elasticity appeared to be injured, as the deflection increased to ·125, and continued to do so until the piece broke with 896 pounds.

According to this result the value of S in my table will be $S = \frac{l w}{4 a d^2} = 1659$, which although less than the tabulated number for this wood, exceeds the average run of oak.

Its elasticity will be (taking $w = 448$ and $\delta = {\cdot}3$ of an inch)

$E = \frac{l^3 w}{a d^3 \delta} = 4609000$, still exceeding the oak. If we adopt the modulus of elasticity according to Mr. Bevan, viz. by expressing the deflecting weight by the length H of a column of wood of the same section and specific gravity as the specimen experimented upon, we have $m = \frac{l^3 H}{4 d^3 \delta} = 3738426$, which is less than Mr. Bevan's number deduced from his experiments on acacia. This, however, was evidently an inferior specimen compared with those employed in the former experiments, being so near the outside as to take in some part of the sap-wood.

Mr. Bevan expresses a wish that I had given the modulus of elasticity instead of the value of E, and observes, "had this been done it would have appeared that the stiffness of Memel Fir, compared with its weight, is greater than for the other woods." I do not, however, with every respect for the well known talents of Mr. Bevan, see what advantage is gained by considering the weight of the timber, except in the particular case where the question is the deflection of a beam from its own weight; a case which very rarely occurs in the construction of buildings.

To give an example of what is stated above, the value of E, or the elasticity of Tonquin Bean, is one and a half time greater than that of Memel Deal; that is to say, it required one and a half time the weight to produce the same deflection, and is accordingly expressed in my table by a number bearing the same proportion; but the modulus is less when we use Mr. Bevan's formula.

It will however be observed, that the two formulæ are established on the same principles; viz. that the deflection varies in the ratio of the cube of the length divided by the breadth into the cube of the depth; so that in all cases Mr. Bevan's number may be obtained from mine, by multiplying by 576, and dividing by the specific gravity.

It is necessary here to mention, that an error in the formula in my former paper may possibly have misled Mr. Bevan. In the head of the sixth column, instead of $E = \frac{l^3 w}{4 a d^3 \delta}$, it ought to have been $E = \frac{l^3 w}{a d^3 \delta}$: the numbers, however, are perfectly correct.

Some other queries of Mr. Bevan, I am sorry I cannot reply to. The price per cubic foot of the different woods I have

no means of obtaining, nor can I state the ultimate deflections; they were not considered of importance, and consequently not registered. With respect to the time, each experiment occupied fifteen or twenty minutes.

VI. *On a new Species of Coloured Fringes, produced by Reflection between the Lenses of Achromatic or Compound Object-Glasses.* By Sir David Brewster, *K.H. LL.D. F.R.S. V.P.R.S. Ed.**

[With a Plate.]

IN a paper which I communicated to this Society in 1815, and which was published in the seventh volume of their Transactions, I described a new species of coloured fringes, produced between two plates of parallel glass. From a consideration of the theory of this class of phænomena, it was obvious that analogous, though much more complicated, systems of rings should be produced between plates with curved surfaces, but it was not till 1822 that I succeeded in detecting them; and so completely are these rings concealed by the superposition of similarly situated images, that, in consequence of having forgotten my method of observation, I have experienced the greatest difficulty in rediscovering them.

My earliest experiments were performed with a double achromatic object-glass, made by Berge, having a diameter of $2\frac{3}{10}$ inches, and 30 inches in focal length. The inner surfaces of the crown and flint glass lenses were ground to different radii, as shown in the section of it at AB, CD, Plate I. Fig. 5; and the outer surface of the flint-glass lens was concave, so that there was left between the lenses a meniscus of air A 2 B 3 A.

In order to observe the system of rings as nearly as possible at a perpendicular incidence, I placed the smallest flame I could procure at S, about four or five inches distant from the object-glass AD, and interposing a small screen G between the flame and the eye at E, I held the eye as close to S as possible, and varied the distance of the object-glass till the inverted greenish-coloured flame† reflected interiorly from the concave surface A 1 B seemed to cover the whole area of the object-glass. When this is accomplished, the rings may, by a slight change in the position of the object-glass, or by screening the image formed by one reflection from A 1 B, be distinctly seen over the expanded but enfeebled image formed by a second reflection from the same surface.

* From the Transactions of the Royal Society of Edinburgh, vol. xii.

† This flame has a greenish colour, in consequence of the rays which form it having passed through twice the thickness of the crown glass lens A B.

When the flame is very small, and the eye sees it projected against the centre of the object-glass, the rings are grouped into a concentric system, as shown in Fig. 1, approaching closer and closer to each other as they advance from the centre to the circumference of the lens. Two of these rings, *mmmm*, *nnnn*, having an intermediate position in the system, are distinguished from the rest by their darkness, and by the whiteness of the light between them; and they enjoy the remarkable property of becoming the bounding lines of *four* systems of fringes, into which the general system is subdivided by oblique reflection.

In order to observe this interesting change, incline the object-glass so that the point A is further from the eye than B, and so that the eye receives the rays that are reflected obliquely from every point of the surface A 1 B. At a very slight deviation from a perpendicular incidence, the rings will become smaller and closer on the side A, and broader and wider on the side B, having intermediate breadths and distances at intermediate points of the circumference between A and B. By increasing the incidence, the inner ring *a a*, Fig. 1, contracts into a sort of irregular crescent *a a*, Fig. 2. The second and third rings, *b b*, *c c*, Fig. 2, do the same as shown at *b b*, *c c*, Fig. 2, and at a greater incidence, the dark ring *n n*, Fig. 1, assumes a similar form *n n n n*, Fig. 2, and forms the boundary of the *remote central system n c b a a b c n*. In like manner, the lower part of the ring *n n*, Fig. 1, has inclosed a smaller but similar system of rings, which are shown at $n' n' n' n'$, and may be called the *near central system*. While these changes are going on, the rings without *n n*, Fig. 1, are undergoing analogous, though opposite, inflections. The outermost *d d d*, Fig. 1, divides itself into two unequal portions, which run out into the circumference at the points $d\ d\ d'\ d'$, Fig. 2. Then the next ring, viz. the dark one, *m m m m*, forming the boundary of the *remote external system m m m*, A, and of the *near central system* $m'\ m'\ m'$ B.

The four groups of rings thus developed, assume, at greater incidences, the character shown in Fig. 3, but they are not seen all at once; and in tracing their form, it is necessary to cause the image on which they are produced, to be reflected successively from different parts of the lens. The rings are so closely packed together at a distance from the centres x, x, to which they are all related, that it is extremely difficult to perceive them. By increasing the incidence still further, the rings close in upon the centres x, x, and become exceedingly close and numerous. The points $x_{,} x$ approach to the circumference of the lens, and the rings become more luminous from

the increase in the reflected light, at increased obliquities of incidence.

In some object-glasses the rings are exceedingly numerous and close, whether seen as in Fig. 1, or as in Fig. 3; and when this is the case, the black rings $m_{\prime} n$, and the centres $x_{\prime} x$, are near the circumference. In other object-glasses, particularly in a large one of Tulley's, in the Calton Hill Observatory, the rings are very few in number, and the dark fringes $m_{\prime} n$, and the centres $x_{\prime} x$, are advanced considerably from the circumference towards the centre of the lens. In this case the rings are more easily seen, and they undergo very beautiful modifications in passing from a perpendicular to an oblique incidence.

There can be little doubt that this variation in the size and number of the rings depends on the thickness of the meniscus of air between the lenses; but in order to put this to the test of experiment I separated the two lenses AB, CD, Fig. 5, and I found the rings to increase in number, and diminish in breadth, in proportion to the distance of the two lenses. Hence it follows, that, in all those object-glasses where the inner surfaces are coincident, or are cemented by mastic or other varnishes, no rings will be produced,—and that the number of the rings furnish us with a measure of the difference of curvature of the inner surfaces of the combined lenses.

In some of the oblique systems of rings which I have observed, the outer fringe n of one of the central systems approached so near the outer fringe m of one of the external systems, that the space between them was *straw-yellow*, in place of white; and in one case, the four bounding fringes united, and formed a black cross, as shown in Fig. 4.

In a large double object-glass, made by Gilbert, 3·8 inches in diameter, and in a similar one by Dollond, 2·75 inches in diameter, the rings could only be seen by looking through the convex side A 1 B, Fig. 5. In the first of these lenses there were only two fringes in the near central system of rings, so that the inner surfaces must have been nearly coincident.

If we separate the lenses a little at A, Fig. 1. and Fig. 5, the system of rings approaches the edge B, and become more numerous and more close to each other. The other systems close, and become concentric to them, and the whole become an elliptical system.

When the lenses are separated a little at B, Fig. 1. and Fig. 5, the system enlarges, and the rings grow more numerous, the other systems becoming concentric with them, and forming a close system.

In a triple object-glass, which gave a system of rings similar

to that in Fig. 3, I observed them to be crossed with another system of minute fringes parallel to one another, and to the line joining the centres x and x. The object-glass which exhibited this curious effect is not now within my reach, so that I am unable to give any further account of this new system.

In order to determine the surfaces of the double object-glass, AD, Fig. 5, which are essential to the production of the rings, I covered the convex surface A 1 B with oil of nearly the same refractive power as glass, and the rings wholly disappeared. Having removed the oil, I filled with the same fluid the space or hollow meniscus between the lenses, when the rings again disappeared. The lenses being again cleaned, I removed CD, and could no longer observe any fringes. Hence, it follows, that the action of the two surfaces of the convex lens, and the inner surface of the concave one, are necessary to the production of the fringes.

From these facts it will appear that the coloured rings arise from the interference of two pencils of light, one of which has suffered *three* reflections within the convex lens AB, and has passed *four* times through its thickness, with another pencil which has suffered *two* reflections within the convex lens, and *one* reflection from the inner surface of the concave lens, and has passed four times through the thickness of the convex lens, and twice through the thickness of the meniscus of air.

When the light is incident perpendicularly on the centre of the lens, the interval of retardation, or the difference between the lengths of the paths of the two rays, is equal to twice the greatest thickness of the meniscus of air. Hence, if this thickness is very small, the tints corresponding to it will be distinctly observed; but if the thickness is considerable, as it often is, the tints will belong to such high orders, that they will only be seen when a small flame of homogeneous light is used.

As the incident ray advances from the centre towards the circumference, the meniscus of air diminishes in thickness, and also the interval of retardation, so that the orders of the rings descend, as in Fig. 1. But there is a particular point between the rings m and n, where the interval of retardation is nothing, or where the lengths of the paths of the two interfering pencils are equal, so that we have a *white* ring at that place. Beyond this, the interval of retardation becomes perceptible, and another system of rings commences, rising to their highest order at the very circumference of the object-glass.

When the eye and the flame are in the axis of the object-glass, the isochromatic lines are circles; but at oblique inci-

dences they have the singular forms shown in Figs. 2. and 3, the line where there is no interval of retardation being the boundary of the four different systems of fringes shown in these figures.

As the paths of the interfering pencils are performed in three media, crown-glass, flint-glass, and air, and as their lengths vary very quickly and irregularly, as the angle of incidence varies, and as the point of incidence changes its position, the analytical expression of the interval of retardation will be very complex.

VII. *Remarks on the Diving of Aquatic Birds.* *By* John Blackwall, *Esq. F.L.S. &c.**

Dr. Drummond of Belfast, in his interesting "Letters to a Young Naturalist," p. 201–202, has the following passage on the diving of water-fowl. "Does a cormorant, or a duck, or a grebe, move more rapidly *under* the surface of water than *on* it? In several parts of Montagu's Ornithological Dictionary, and the still more valuable Supplement to it, you will find illustrations on this point, showing that the same power will cause a much more rapid motion in diving than in swimming; and the cause is this:—When a bird moves in water, or upon it, there is a movement upwards as well as forward; but in swimming, the momentum upwards is lost, and the bird derives benefit only from the forward impulse. But in diving, the pressure of the water above prevents the ascending movement, and consequently the impetus is not lost, as if the bird were on the surface, and therefore the propelling power is greater; and the bird moves faster, because, in diving, the whole moving power is effective; whereas in swimming, a part of it is lost, and the progress is proportionally lessened."

Many years since, when perusing for the first time the observations on the diving of aquatic birds contained in the Introduction to the Ornithological Dictionary, p. xxxix.–xl., the insufficiency of Montagu's attempt to solve this problem was perceived, and I was induced to make a few comments on the subject in my zoological note-book: it is probable, however, that they never would have filled a more conspicuous situation than that which they have so long occupied in its pages, had not my attention been again directed to them by Dr. Drummond's recent introduction of Montagu's hypothesis in a work professedly written for the instruction of young persons

* Communicated by the Author.

who are commencing the study of natural history. Scrupulous accuracy must always be regarded as an object of primary importance in a publication of this description, and I feel confident that Dr. Drummond has too much candour and good sense to be offended with the correction of an error into which he has been inadvertently led by the hasty adoption of a doctrine which is directly opposed to the established principles of dynamics.

It is asserted by the advocates of this doctrine, that the action of the legs in diving not only gives to birds a progressive motion, but also a tendency to rise, which tendency being overcome by the pressure of the water above them, the entire moving force is directed in the line of the body, accelerating thereby the velocity with which they pursue their subaqueous course.

Now it is a law of hydrostatics, that the pressure of fluids in a state of equilibrium is equal in all directions at the same depth; whatever obstacle, therefore, the circumstance of pressure may present to the ascent of a bird when diving, it must also present, *cæteris paribus*, to its progressive motion. Moreover, it is manifest from the exceeding facility with which the particles of water move among one another, that if any tendency upward did result from the action of the limbs of waterfowl in diving, it could not be wholly counteracted by the pressure of the mass of fluid above them; indeed, the specific gravity of such birds being less than that of water, it would not be possible for them to continue beneath its surface, even for a much shorter period than they are known to do, without the employment of physical force to effect their purpose; hence the fallacy of the argument, that the propelling power is increased on such occasions by the pressure of the superincumbent water, is rendered sufficiently obvious.

It remains to consider what means are actually made use of by birds in diving to overcome the resistance of the medium in which they move, and the tendency upward arising from their small specific gravity; and as Mr. White has illustrated this subject in his usual felicitous manner, in treating upon the Northern Diver, (*Colymbus glacialis*,) Linn., in the second volume of the octavo edition of his Works in Natural History, p. 184–186, I cannot do better than avail myself of his observations.

"Every part and proportion of this bird" (the Northern Diver) "is so incomparably adapted to its mode of life, that in no instance do we see the wisdom of God in the creation to more advantage. The head is sharp, and smaller than the part of the neck adjoining, in order that it may pierce the

water; the wings are placed forward and out of the centre of gravity for a purpose which shall be noticed hereafter; the thighs quite at the podex, in order to facilitate diving; and the legs are flat, and as sharp backwards almost as the edge of a knife, that in striking they may easily cut the water; while the feet are palmated, and broad for swimming, yet so folded up when advanced forward to take a fresh stroke, as to be full as narrow as the shank. The two exterior toes of the feet are longest; the nails flat and broad, resembling the human, which give strength and increase the power of swimming. The foot, when expanded, is not at right angles to the leg or body of the bird; but the exterior part inclining towards the head forms an acute angle with the body; the intention being not to give motion in the line of the legs themselves, but by the combined impulse of both in an intermediate line, the line of the body.

"Most people know, that have observed at all, that the swimming of birds is nothing more than a walking in the water, where one foot succeeds the other as on the land; yet no one, as far as I am aware, has remarked that diving fowls, while under water, impel and row themselves forward by a motion of their wings, as well as by the impulse of their feet; but such is really the case, as any person may easily be convinced, who will observe ducks when hunted by dogs in a clear pond. Nor do I know that any one has given a reason why the wings of diving fowls are placed so forward: doubtless, not for the purpose of promoting their speed in flying, since that position certainly impedes it; but probably for the increase of their motion under water, by the use of four oars, instead of two; yet were the wings and feet nearer together, as in land-birds, they would, when in action, rather hinder than assist one another."

Mr. White's description of the manner in which the Northern Diver impels itself through the water by the agency of the legs, which have an extent of motion enabling it to alter its course in any direction whatever with astonishing facility, is applicable to diving-birds in general; but it does not appear that the wings are so uniformly employed to promote their progress when submerged, as the statement of the natural historian of Selborne would seem to imply.

I may remark, in conclusion, that the action of the legs in diving, so far from giving birds an impulse *upward* and forward, as Montagu has affirmed, evidently tends rather to propel them *downward* and forward, except when they purpose to ascend, and then a change of action, adapted to the accomplishment of the object to be attained, is instantly re-

sorted to The simultaneous action of the legs also, directing the impelling power in the line of the body, will explain why the velocity with which aquatic birds move in so dense a fluid as water is greater than that with which they move on its surface, where the legs are usually employed alternately, and the moving force cannot be so advantageously applied; and that the velocity is frequently accelerated still further by the instrumentality of the wings, has been already noticed.

Thus, in controverting the erroneous opinions of Montagu relative to the diving of water-fowl, I have endeavoured to substitute for them a satisfactory theory of this remarkable phænomenon.

Crumpsall Hall, April 18th, 1832.

VIII. *Addition to the Analytical Investigation of a Formula for the Relative Importance of the Boroughs; (contained in the March Number of the Phil. Mag. and Annals). And Reply to Dr.* M'Intyre's *Remarks. By the* Author of that Investigation*.

BY referring to the above-mentioned investigation, it will be found that putting

H for the whole houses in all the boroughs;

T the whole sum paid in assessed taxes;

B the numerical value of their united relative importance;

Also, putting h, t, b for the same of any one of the boroughs; —the relative importance of a borough may be expressed by the formula

$$b = \mathrm{B}\left\{m'\frac{h}{\mathrm{H}} + n'\frac{t}{\mathrm{T}}\right\} \text{............} \quad (1)$$

in which B may be any number whatever, and m' and n' are numbers which serve to adapt the formulæ to some hypothesis concerning the relation which House-importance bears to Tax-importance.

Such as have taken an interest in the question will recollect that two rules for estimating the importance of a borough were proposed: one was that of LieutenantDrummond, which was adopted by the Government in framing the Reform Bill; and another, that proposed by Mr. Pollock in the House of Commons, in opposition to Mr. Drummond's: this was, in fact, the same in its operation as one proposed by Dr. M'Intyre, in December of last year, to Lord Melbourne. The propounders of these rules all asserted that they were founded on just mathematical principles: it might therefore be a ques-

* Communicated by the Author.

tion, whether there could be more than one formula deducible from the data? which were the houses and taxes of the boroughs.

The writer of this article had satisfied himself as well as others, by the dictates of common sense, that Lieutenant Drummond's rule was correct. He, however, judged that it might be proper to discuss the subject upon mathematical principles that should be beyond dispute, and he chose the very simple axiom, "That if a town contain as many houses as are in any number of boroughs, and pay as much in taxes as they all pay, its importance will be equal to the united importance of all these boroughs." From this he formed a functional equation, and employing the Differential Calculus (not the *Calculus* of Variations, as Dr. M'Intyre supposes)*, he determined the *form* of the function, and proved beyond dispute that it could have but one form, which is that of formula (1) of this article.

Lieutenant Drummond has not explained the views by which he was led to his practical rules, given in the parliamentary paper (see page 219 of the Phil. Mag. and Annals for March). It is easy to see, however, that by making $m' = n'$ in the general formula (1), so that it becomes

$$b = m' \mathrm{B} \left\{ \frac{h}{\mathrm{H}} + \frac{t}{\mathrm{T}} \right\},$$

we have immediately Mr. Drummond's rule. It is true the constant factor m' here put down, was left out in the former communication, because it did not in the least affect the position of the boroughs in the scale of comparative importance, and the number B was supposed to be 1000000, to avoid fractions in the results: but it was not expected that any one would lay hold of so trivial a matter, in order to attempt to show that Lieutenant Drummond's rule really involved the absurdity that two is equal to one. It appears, however, that the candour of his opponents was overrated: the objection of Dr. M'Intyre is a fair specimen of it. The Doctor stands exposed to some sharp remarks: but at present this purely scientific speculation shall not be contaminated with any thing personal.

We shall now proceed to show, by carrying the analysis a step further, that the Doctor's objection does not apply to the general formula

$$b = \mathrm{B} \left\{ m' \frac{h}{\mathrm{H}} + n' \frac{t}{\mathrm{T}} \right\}.$$

Since this must hold true whatever be the magnitude of a

* Dr. M'Intyre's paper will be found in Phil. Mag. and Annals, vol. xi. p. 360.—Edit.

borough, let us suppose h = H and t = T; then we ought to have b = B, and therefore

$$\mathrm{B} = \mathrm{B}\left\{m' \frac{\mathrm{H}}{\mathrm{H}} + n' \frac{\mathrm{T}}{\mathrm{T}}\right\};$$

Hence it appears that $m'+n' = 1$: This condition will be satisfied if we make

$$m' = \frac{m}{m+n}, \qquad n' = \frac{n}{m+n}:$$

Our general formula now becomes

$$b = \frac{\mathrm{B}}{m+n}\left\{m \frac{h}{\mathrm{H}} + n \frac{t}{\mathrm{T}}\right\} \text{............} \quad (2)$$

Where, as before, m and n are conventional numbers, which are to satisfy the hypothesis of some supposed relation between the house- and tax-importance: if these are to have *equal weight*, then $m = n$, and the formula becomes

$$b = \frac{\mathrm{B}}{2}\left\{\frac{h}{\mathrm{H}} + \frac{t}{\mathrm{T}}\right\} \text{......................} \quad (3)$$

Dr. M‘Intyre may now try his criterion of absurdity on either of the formulæ (2) (3); namely, the supposition that B = $b+b'$, H = $h+h'$ and T = $t+t'$; and he will find that they both stand the test. But in his remarks, he has also required that our formula should at the same time satisfy the two conditions

$$\left.\begin{aligned} b &= \frac{\mathrm{B}}{m+n}\left\{m \frac{h}{\mathrm{H}} + n \frac{t}{\mathrm{T}}\right\}, \\ \mathrm{B} &= \frac{b}{m+n}\left\{m \frac{\mathrm{H}}{h} + n \frac{\mathrm{T}}{t}\right\}. \end{aligned}\right\}$$

Let us suppose, if possible, that they satisfy these conditions; and, taking the product of the two equations, and leaving out common factors, &c. we obtain

$$(m+n)^2 = m^2 + n^2 + mn\left(\frac{h}{\mathrm{H}} \cdot \frac{\mathrm{T}}{t} + \frac{\mathrm{H}}{h} \cdot \frac{t}{\mathrm{T}}\right);$$

and hence again,

$$2 = \frac{h}{\mathrm{H}} \cdot \frac{\mathrm{T}}{t} + \frac{\mathrm{H}}{h} \cdot \frac{t}{\mathrm{T}};$$

and $$2 \frac{h}{\mathrm{H}} \cdot \frac{t}{\mathrm{T}} = \left(\frac{h}{\mathrm{H}}\right)^2 + \left(\frac{t}{\mathrm{T}}\right)^2;$$

and $$\left(\frac{h}{H} - \frac{t}{T}\right)^2 = 0;$$

and at last $$\frac{h}{H} = \frac{t}{T} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (\alpha)$$

Now, if h and t be the houses and taxes of the medium borough, the condition will be exactly satisfied; and it is certain that h' and t' being the houses and taxes of any one of the boroughs, we have nearly

$$\frac{h'}{h} = \frac{t'}{t}; \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (\beta)$$

Therefore, compounding the ratios α, β, we have in all the boroughs

$$\frac{h'}{H} = \frac{t'}{T}, \text{ nearly.}$$

Thus it appears that our general formula satisfies all the conditions which our most strenuous opponent has required, as far as the thing is possible.

It has been admitted that a more powerful instrument of analysis was employed in the investigation than was absolutely necessary, (see p. 223 of the Number for March). Neither the learned Doctor, nor the *men* of *Trinity*, have however complained of this. But the very same conclusions may be obtained by the ordinary algebraic analysis, from the simple common-sense principle, that "the importance of a borough may be truly expressed by giving a certain numeral importance to each house, and a certain numeral importance to each pound, paid in taxes;" just as we would estimate the share of political influence due to the possessor of an estate from his annual income, found by adding into one sum his rents derived from land of different qualities,—say arable and pasture. Proceeding on this principle:

Let x denote the numeral importance of a house,

y that of one pound paid in taxes.

Then, B, H, T, b, h, t denoting the same as before, we have

$$b = xh + yt = x\left(h + \frac{y}{x}t\right);$$

$$B = xH + yT = x\left(H + \frac{y}{x}T\right);$$

In the second equation, xH and yT express the whole relative importance of the two elements, houses and taxes. We may make any hypothesis we please concerning the power of each separately to raise the importance of the borough. Let us suppose that their powers are to each other as a given num-

ber m to a given number n; that is, let $x\text{H}:y\text{T}::m:n$; we have now a third equation;

$$\frac{y}{x}=\frac{n\text{H}}{m\text{T}}.$$

Which being substituted in the other two, they become

$$b=\frac{x\text{H}}{m}\left\{m\frac{h}{\text{H}}+n\frac{t}{\text{T}}\right\},$$

$$\text{B}=\frac{x\text{H}}{m}(m+n):$$

and from these, by division, &c. we get

$$b=\frac{\text{B}}{m+n}\left\{m\frac{h}{\text{H}}+n\frac{t}{\text{T}}\right\}.$$

The same formula as was found by a very different process.

The controversy concerning the just rule for estimating borough importance may be explained briefly as follows:

All parties agree in deducing it from the two fractions

$$\frac{h}{\text{H}},\quad \frac{t}{\text{T}}.$$

Lieutenant Drummond has placed the boroughs in the scale of relative importance by the formula

$$b=\frac{1}{2}\left\{\frac{h}{\text{H}}+\frac{t}{\text{T}}\right\};$$

or at least by a rule equivalent to this formula.

Mr. Pollock proposed the formula

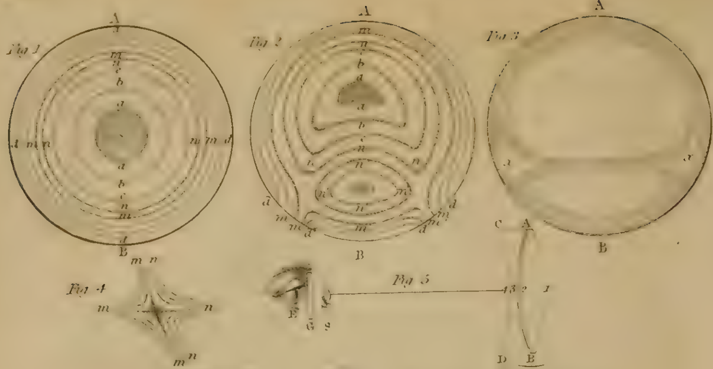
$$b=\sqrt{\frac{h}{\text{H}}\cdot\frac{t}{\text{T}}}.$$

Dr. M'Intyre strongly insists that the true formula should be

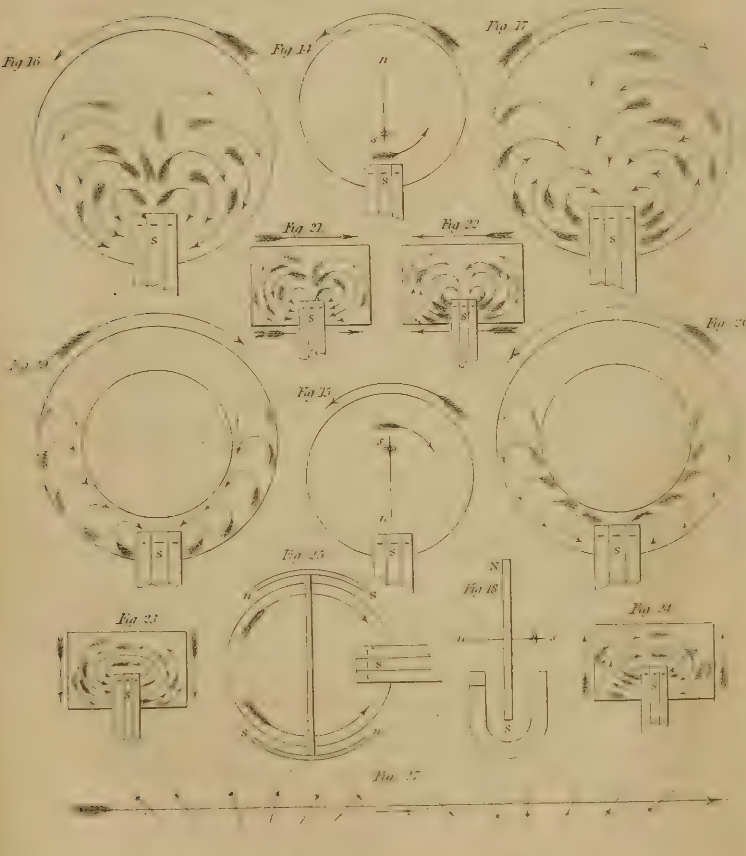
$$b=\frac{h}{\text{H}}\cdot\frac{t}{\text{T}}.$$

The two last will place the boroughs exactly in the same order. The first, however, is the only rule founded on true principles: the others have been formed from confused or mistaken notions about the doctrine of ratios.

The question comes at last simply to this:—shall we take an arithmetical or a geometrical mean between the two fractions $\frac{h}{\text{H}}$ and $\frac{t}{\text{T}}$ as the measure of the importance of a borough? In a practical point of view it hardly signifies which rule be followed; yet it would have been discreditable



Sir David Brewster on Fringes.



Mr. Sturgeon's Magnetic Experiments.

to the Government to have thrown aside the rule of Lieutenant Drummond, founded on true principles, and taken in its stead Dr. M'Intyre's, which satisfies no mathematical principle whatever. G. V.

IX. *On the Distribution of Magnetic Polarity in Metallic Bodies. By* W. Sturgeon, *Lecturer on Experimental Philosophy at the Honourable East India Company's Military Academy, Addiscombe.*

[With a Plate.]

IN my last communication (Phil. Mag. and Annals, N.S. vol. xi. pp. 270, 324,) I described the instrument (fig. 9. Plate III. vol. xi.) by means of which the experiments were first made, which indicated an extraordinary and novel distribution of magnetic polarity on the surfaces of copper and other non-ferruginous metallic discs. I also pointed out, though briefly, the method by which I detected the curiously winding force which actuates the needle on those surfaces when rotated between the poles of a horse-shoe magnet. In that communication, however, I described the distribution of that force no further than as it is developed by one condition of motion given to the disc; i. e. whilst it rotates in the direction of the large exterior arrow, fig. 11. Plate III. In continuation, therefore, I now proceed to show in what manner that force (still supposing it to be the electric) becomes distributed over the surface of the copper disc, when the rotation is carried on in the reverse order; the magnet still remaining in the same position as in fig. 11. Plate III. It may be necessary, however, in the first place, to make some further observations as to the manner by which I have been enabled to trace the curious windings which this force appears to take whilst in operation on the magnetic needle, or the mode by which I obtained the data necessary to the formation of the conclusions at which I have arrived concerning it.

In Experiments 16. and 17. it is shown that when the needle is placed over the centre of the disc, and its axis in the same vertical plane as that joining the poles of the exciting magnet, it is a matter of no consequence in which of the directions the poles of the needle be placed; the deflections will depend upon the direction in which the disc is caused to rotate. For, although the needle will in some cases follow the direction of the rotating disc, and in others travel the contrary way, according to the character of the pole which is directed towards the pole of the exciting magnet, still it will have a dependence upon the direction of motion given to the plate; so that if the

position of the needle be such that its deflections will correspond with the motion of the plate when rotating to the right, its deflection will again correspond with the motion of the disc when the latter is caused to rotate to the left; consequently the deflections in the first case will be contrary to the deflections in the latter case. The same law will be observed when the position of the needle is such that the deflections are opposed to the direction of motion given to the disc; for if the needle travel towards the left whilst the disc revolves towards the right, it will travel towards the right when the revolution of the plate is towards the left; manifesting in all cases that when the exciting magnet is stationary, the direction of the force which impels the needle entirely depends upon the direction of motion given to the disc.

This law being understood, we have next to contemplate the direction in which any selected pole of the needle travels whilst the disc is in motion; and a little reflexion will make it readily appear that in whichever of the two positions (Experiments 16. and 17.) the needle may be placed whilst the plate is at rest, it will exhibit a *constant tendency* to assume some *determined new position* when the motion given to the plate is in *one certain direction;* and as constant a tendency to take up *some other* determined new position when the rotation of the plate is reversed.

To simplify this point still more, we will first suppose the needle to be placed as in fig. 14. Plate I., *s n* being respectively the south and north poles. The needle in this position will travel in the same direction as the disc revolves (Experiment 16.); and when the revolution of the disc is in the direction indicated by the large exterior arrow, the *south pole* of the needle will be deflected towards the *right* of the exciting magnet. Again: Let the needle be placed as in fig. 15. *s n* as before being the south and north poles respectively. In this case the needle will travel in the opposite direction to that of the revolving disc (Experiment 17.); but in this, as in the former case (as will be observed by comparing the two figures), when the disc revolves in *one* and *the same* direction, as indicated by the large exterior arrows, the *south pole* of the needle has a constant tendency towards the right hand, as is shown by the small arrows pointing out the direction of its course. Hence the *new position* for the south pole of the needle, determined by the forces excited in the disc by its revolving in this particular direction, is evidently on the right side of the exciting magnet, or to the right of an observer with the apparatus placed before him, as in fig. 14 and 15.

This point being ascertained, the needle is now to be

arranged, first a few degrees from the one, and then a few degrees from the other of its former positions, still keeping the *south pole* towards the right. The disc is to be put in motion (in that direction only indicated by the large exterior arrows, fig. 14. and 15.), whilst the needle is in each of the positions last given to it; and if the south pole now travels in the same direction as it did from both its former positions, it is plain that the excited forces still urge it towards some point, the situation of which is between those in which it was last placed. The needle is therefore again to be drawn still nearer to its destination indicated by the last trials, and the disc again put in motion in the same direction as before; the deflections are again to be observed, and the line, to which they indicate a tendency, to be still nearer approached by the position of the needle, for the next trials. In this manner the line to which the excited forces of the disc urge the needle is to be gradually approached, and its true position at length correctly ascertained.

The deflections will gradually diminish, becoming smaller and smaller in proportion to the advances of the needle towards this *neutral line:* and when it is placed directly in the position of this line, the deflections will cease to be exhibited by the direction of rotation selected for this illustration; for the needle will now have a position of stability, or a position which the forces excited in the disc alone tend to preserve it in. If it be drawn only two or three degrees out of this line on either side, the slightest motion of the disc will urge it towards that line again; and if the needle be made completely indifferent to the influence of any other forces than those excited in the disc, a deviation even of one degree on either side of the neutral line may be detected by a tendency which will be indicated to resume the position of that line again whenever the plate is rotated in the proper direction*.

The process of experimenting is exceedingly tedious, but it is the only method by which the true position, to which the forces excited in the disc tend to urge the needle, can possibly be ascertained. And if those forces be electric, and endued with the same magnetic polarity as that exhibited by the forces of a galvanic conducting wire, then the directions of the electric

* I have been particular during this description in adhering to the effects of those forces which become excited by the disc revolving in one direction only; because it so happens that the two neutral lines indicated by the needle whilst placed over the centre of the disc are not coincident, but intersect each other at some considerable angle. Hence, although a position may be given to the needle from which it will not deviate whilst the plate revolves in one direction, a considerable deflection may be given by reversing the rotatory motion.

tides on the surface of the disc will be at right angles to the several positions which the needle is thus found to assume whilst the disc is in rotatory motion; and it was from numerous experiments and observations of this kind, whilst the needle was placed over various parts of the surface, that the necessary data were discovered, and the recurving forces carefully traced out.

The process by which the distribution of polarity on the surface of the disc has been determined being now understood, no further explanation will be necessary to illustrate the singular recurving directions of the excited forces which are supposed to actuate the needle on the upper surface of the disc, under the two conditions of rotation, than merely to refer to fig. 16.* and 17. The exterior arrows indicate the directions of motion given to the disc; and the two systems of small recurving arrows in each figure show the distribution and direction of the forces which impel the needle and urge it to a position at right angles to the aggregate of any portion of those forces over which it may be placed during the revolving motion of the disc.

It will be observed, by comparing fig. 16. and 17, that the direction in which the aggregate of the forces recurves is nearly if not completely reverse by simply changing the revolving motion of the disc. The arrows which indicate the direction of those forces are seen to issue from the front of the exciting magnetic pole in fig. 16, but are re-entering at that point in fig. 17. In the former figure also, the arrows are seen re-entering on both sides of the magnet, near to the edge of the disc; but in the latter figure the arrows issue forth from both sides of the magnet, along the same edge; so that the force in the edge of the disc is as decidedly reversed as it is in any part of the area by simply reversing the revolving motion. The curious change in the direction of the force in the edge of the disc is beautifully illustrated by the following experiment.

Experiment 20. Let the axis of the disc be placed horizontally east and west, and consequently the plane of it will be coincident with the plane of the meridian. Let the horse-shoe magnet be so arranged as to embrace the south edge of the disc between its poles, its plane horizontal, and coincident with that in which the axis of rotation is situated. Let also

* Since my former communication went to press I have had an opportunity of repeating my experiments on the surface of the disc; from the results of which I have been induced to offer fig. 16. as a more faithful representation of the distribution of the force in the central parts than that which is shown by fig. 11. (Plate III.) vol. xi.

the north pole be opposite the east surface, and the south pole opposite the west surface of the disc, as in fig. 18, S N representing the upper edge of the disc.

Let a magnetic needle be also arranged north and south, close beneath the lower edge of the plate. Rotate the plate in the direction which will carry its upper edge from south to north, (from S to N, fig. 18.). In this case the disc will enter between the poles of the exciting magnet, under precisely the same circumstances as in fig. 11. and 16; the left edge in either of those figures corresponding with the *lower edge* in fig. 18. The principal force which now operates on the needle will be that in the lower edge of the disc; and the direction of that force will be from north to south, or in the same direction as that in which the lower edge is in motion. (See fig. 11. or 16.) The south pole of the needle is deflected towards the *east* in precisely the same manner as it would be urged by the polarizing force of an electric current running from north to south through a conducting wire placed above the needle. The needle S N, fig. 18, shows the position into which it is carried whilst the disc is revolving over it.

Experiment 21. Let the needle be now placed above the upper edge of the disc, and its axis in the same vertical plane, the rotation being continued in the same direction as before. In this case the force which operates on the needle is transmitted from north to south, the upper edge of the disc corresponding to the *right* edge in fig. 11. or 16. The direction of the force is therefore the same in this experiment as in the last; but the needle is now placed *above* the edge, and the south pole is deflected towards the west.

If the disc be rotated in the contrary direction to that in which it proceeded in the two last described experiments, the distribution of the force will be represented by fig. 17, in which case its direction in the edge, both above and below the magnet (fig. 18.), will be from south to north. The south pole of the needle, when beneath the *lower* edge, will be deflected towards the west; but when placed above the *upper* edge of the disc, the same pole will be deflected towards the east; showing in a very beautiful and striking manner that the forces in the edge of the disc become completely reversed by simply reversing the revolving motion, and that the distribution of polarity is highly imitative of that which is displayed by the edges of a flag or cake of zinc, when partially heated at one end only *; the discovery of which, as I have before stated, gave me the first hint which led to the

* See my Paper in the Phil. Mag. and Annals, vol. x. p. 120.

success at which I arrived in the investigation I am now describing*.

If two or three discs of the same diameter be placed close together on the same axis, so as to form a compound disc or plate, the forces which operate on the needle are much more powerful than when one disc only is employed. Much, however, depends upon the thickness of the metal, thick discs having a great advantage over those which are very thin; notwithstanding which, a decided uniformity in the distribution of polarity is displayed even in the thinnest copper or zinc foil.

I made a compound disc by soldering the edges of two single ones to a rim or hollow cylinder of copper, about half an inch deep, so that when completed it formed a cylindrical box, half an inch high, and about ten inches in diameter, having a perforation through its centre for the introduction of the spindle on which it was intended to rotate. When this cylinder was mounted in the place of the single disc in experiment 20. and 21, the deflection of a four-inch needle (neutralized in the usual way) would amount to about 40° with a moderate velocity of rotation. When the velocity was considerable, and the motion equable, the needle would be perfectly steady at that, or at a greater angle of deflection.

Straight needles, particularly when they are very long, are by no means well adapted for obtaining the greatest effect from the forces in the edge of the disc whilst rotating in a vertical plane, because of the great distance at which the poles are necessarily placed from those operating forces. It is much better to employ needles which are bent into circular arcs, having nearly the same curvature as the edge of the disc. Two needles of this form may be advantageously employed at the same time; the one above, and the other below, and both concentric with the edge, as in fig. 25. The needles are attached to a straw, or thin slip of light wood, with their poles placed in opposite directions. When thus arranged, their directive force will, in a great measure, be neutralized, both as regards the magnetism of the earth, and that of the exci-

* At the time I was making these experiments, I found that the frame of an electrical machine with a multiplying wheel and band was very convenient for giving the disc a considerable velocity in a vertical plane. A spindle, supported in the pivot-holes of the frame, and furnished with a pulley at one end, carried the revolving disc; and a pile of books formed the stage for the support of the horse-shoe magnet. Some time last summer, however, I constructed an apparatus for the purpose of rotating discs, cylinders, &c., on a horizontal axis, which, as it very much resembles a *plate machine*, it is not necessary to describe in this place,—any further than merely to mention, that it is furnished with neat stages for the support of the exciting magnet and the compass-needles.

ting horse-shoe; and as the actuating forces in the edge of the disc operate in the *same* direction, both needles will be impelled in one and the same way; so that whatever may be their position when deflected, they will constantly appear in the same vertical plane. The arrows in fig 25. show the direction of the aggregate forces in the edge of the disc, when it is rotated in the direction as shown in fig. 16.

The singular and complicated distribution of the force discovered in these rotating discs of copper, led me to undertake some other experiments, by means of which I considered it possible that I might arrive at some simple law, which would disclose the novel and apparently mysterious arrangement; for, whether the phænomena emanate from magnetic or from electro-magnetic action, there appeared to me to be no law yet discovered in either of these branches of research, that would produce a distribution of polarity like that which I have portrayed in fig. 16. and 17; notwithstanding which, the uniformity of the distribution, which became manifest at every repetition of the experiments, left no doubt as to the immutability of some law, to the operation of which the regularity of the distribution was entirely owing.

In this investigation it was necessary to take into consideration the various directions which different parts of the revolving disc assume with regard to the exciting magnet; for, as the poles are not placed in the centre of motion, it is plain that whilst some parts are advancing towards them, other parts are receding from their vicinity;—some parts again are crossing the magnet to the right, whilst others are crossing it towards the left; and all these motions in the disc are going on at the same time; so that upon the whole the apparent complexity of the problem put any inquiry concerning it rather in the position of a "forlorn hope", than of anything like certainty of success.

Considering, however, that as the vicinal regions of the disc must necessarily receive the exciting impressions in a much higher degree than those more remotely situated from the magnetic poles, it might be expected that if any satisfactory conclusions were to be arrived at, those parts of the disc the most powerfully excited were more likely than any other to afford the necessary data. My inquiries were therefore more particularly directed to the investigation of that *half* of the disc which is nearest the magnet, the curvilinear direction of which, with regard to the exciting pole, is easily resolved into four rectilinear motions.

Let *m o*, fig. 26*, be the constant radius situated between

* In consequence of an oversight, fig. 26. will not be found in the plate; it will be given in our next Number.—Edit.

the magnetic poles; then the diameter $n\, n'$ drawn at right angles to the former line will be the line of demarcation which separates the disc into the two required halves.

Now when the disc revolves in the direction of the exterior arrow, the quadrantal portion $m\, o\, n$ will advance towards the pole m; whilst the quadrantal portion $m\, o\, n'$ will recede from it.

Let $c\, o$ be any radius of the disc approaching the magnet m; then, in order that any point c in that line may arrive at m, it must necessarily partake of the direction $c\, b$, which would bring it towards the *side* of the magnet; and also of the direction $b\, m$, which would carry it to within the magnetic poles: and as the lines $c\, b$ and $b\, m$ are respectively the exact measures of the spaces through which the point c would have to travel in those directions, whilst approaching the magnet, and are both performed in the same time,—they are also the faithful representatives of the respective *mean* velocities with which the point c is carried in each direction whilst advancing from c to m.

Now as $c\, b$ and $b\, m$ are respectively the sine and versed sine of the angle $c\, o\, m$, the mean velocity from c to m in each direction will always be proportional to those lines, from whatever point of the quadrant the point c has to travel. If c travels through an arc of 90°, or from n to m, the mean velocity in each direction will be equal, because $n\, o = o\, m$; but if the arc be less than 90°, the mean velocities will be unequal. If the arc $n\, c$ be 45°, the mean velocity from n to c will be in favour of the direction $o\, m$; but between c and m the predominating velocity will be in the direction of $c\, b$.

Now as the excitation is more powerful in the neighbourhood of the magnetic poles than in any other part of the disc, the vicinal area $c\, o\, m$ of the quadrant $n\, o\, m$ will constantly be receiving stronger impressions than the remote area $n\, o\, c$. And as the predominating mean velocity in the area $c\, o\, m$ is in the direction $c\, b$, the ascendant influence will consequently be due to that direction of motion.

With regard to the quadrantal area $m\, o\, n'$, nothing more appeared necessary to be understood than to resolve its curvilinear motion into rectilinear directions in the manner already considered in the other part of the disc, supposing it to be receding from the magnet, instead of advancing towards it.

Under these considerations the experiments necessary for the inquiry, which at first view had appeared to present considerable difficulty, became very much simplified, being reduced to four rectilinear motions of the plate;—attending to the velocity in each direction, and taking into calculation the observed phænomena under each individual circumstance.

The experiments were made with a rectangular plate of

copper, about 18 inches long and 12 inches broad. This plate was placed between the poles of a horse-shoe magnet, and moved in a horizontal plane. The upper surface of the plate was exposed to the action of the *south* pole, and consequently the lower surface to the action of the *north* pole of the magnet.

Nothing more will be necessary to describe the distribution of the force which operates on the needle, whilst the plate is in motion in the four selected directions, than merely to refer to figures 21, 22, 23, and 24. The exterior arrows in each figure indicate the direction in which the plate is moved; and the curved systems of arrows show the distribution of the force.

In fig. 21. the distribution is similar to that shown in fig. 11. or 16; and the motion of that part of the metal under the strongest excitation in both cases is in the same direction, i. e. from left to right. The same comparison may be made between fig. 22. and 17, where both move from right to left between the poles of the exciting magnet.

In fig. 23. the plate is introduced directly into the interior between the two limbs of the magnet; and in fig. 24. it is withdrawn in the same right line. The distribution of the forces by these two motions are simple curves, having only one direction in each. In each case, however, the curves have every appearance of being continuous, running into themselves between the poles of the magnet, and forming complete vortices round a central nucleus or narrow space joining the exciting poles.

Now as the distributions in fig. 23. and 24. are simple vortices, they may be applied to explain the compound distributions in the other figures. Let it be supposed that each system of arrows in fig. 23. and 24. represents a complete vortex of the force, and let an observer be supposed to be placed in its centre; then as the plate advances towards the poles as in fig. 23, the direction of the force in every point of the vortex will be towards the left hand; but when the plate recedes from the magnetic poles, as in fig. 24, the direction of the force will be towards the right hand. These are simple elementary vortices.

Apply now each of these elementary vortices to fig. 21. and 22. In each figure the plate is both advancing and retiring from the pole at the same time. In fig. 21. the plate is advancing on the left side of the magnet, and the vortex flows towards the left hand of an observer placed in the centre of its motion. On the right side of the magnet the plate is retiring from the poles, and the vortex is flowing towards the right hand, or in the same direction as in the elementary vortex in

fig. 24. In this way the elementary vortices in fig. 23. and 24. will explain the compound distributions of force in each individual case, as represented in the figures.

In fig. 16. and 17, where the disc revolves on a centre, the excitation arising from the motion being in the direction *o m* on one side of the magnet, fig. 26, is counteracted by the opposite excitation on the other side of the line *o m*; for as on one side of the magnet the motion would be advancing, and on the other side retiring, as in fig. 23. and 24. respectively, the forces arising therefrom would nearly, perhaps completely, destroy each other. It is possible, however, nay it is even probable, that all the systems of forces arising from the four rectilinear motions are in play when the disc is revolving on its axis; but the insignificancy of the two last contemplated forces, with regard to those which are due to the motions indicated by fig. 21. and 22, must necessarily render them exceedingly inefficient. If the force be electric, it is likely that the remote parts of the disc serve merely as conductors to that excited in the parts vicinal to the magnet.

The small curved arrows in fig. 19. and 20. indicate the distribution of the force in annular discs of copper or zinc, when rotated on an axis in the manner described for complete discs. The large exterior arrows indicate the direction of motion in each figure. The distribution in these annular discs is precisely the same, so far as the metal permits, as that in complete discs.

Fig. 27. is intended to show the position of the neutral line on the rectangular plate, when moved in the direction of the arrow between the magnetic poles. The arrow is a right line crossing the magnetic pole, and two inches in front of it. The small needles are placed an inch from each other, and their positions, with regard to the arrow, show the inclination at each station, or the position in which the excited forces in the plate alone would place them.

X. *On the Resistance to the Motion of small Spherical Bodies in elastic Mediums. By the Rev.* J. Challis, *Fellow of the Cambridge Philosophical Society**.

THE following observations have reference to the communication I made to the Philosophical Magazine and Annals for last March, and the mathematical reasoning therein contained, which being of a novel kind, requires to be con-

* Communicated by the Author.

firmed in every possible way. I shall here attempt to show that the results of that reasoning will serve to explain a phænomenon, which, as far as I know, has not yet received explanation.

These results were such as follow. If a disturbance be made in an elastic medium, in which the pressure is equal to the product of a constant (a^2) by the density (ϱ), by means of a small sphere, the surface of which vibrates while its centre is fixed, and if $v =$ the velocity at the time t, at any point either at the disturbing surface or indefinitely near it, distant from the centre by r, then,

$$v = \frac{F'(r-at)}{r} - \frac{F(r-at)}{r^2}$$

$$a \text{ Nap. log } \varrho = \frac{F'(r-at)}{r}.$$

The former of these equations shows that v is made up of two parts, $\frac{F'(r-at)}{r}$, and $-\frac{F(r-at)}{r^2}$, distinguished from each other by the denominators r and r^2. These denominators show that the velocity varies in passing at a given instant from the disturbing surface to a point indefinitely near, in a manner independent of the arbitrary function, and therefore of the disturbance also. We may perceive a natural reason for this, by considering that as the surface expands, the number of particles in contact with it is continually increasing, and to supply the increase the contiguous particles must have a motion towards the centre, independent of the motion they receive from the surface; and similarly when it contracts, a motion from the centre. Because a Nap. log ϱ is also equal to $\frac{F'(r-at)}{r}$, it was inferred that this part of the velocity is propagated with the uniform velocity a. The other part, not being accompanied by change of density, is transmitted instantaneously, as if the fluid were incompressible.

We considered the case in which $F(r-at) = m \sin \frac{\pi}{\lambda}(r-at)$, which applies to vibratory motion. Let us suppose for greater generality that $F(r-at) = m \times \sin \frac{\pi}{\lambda} \phi(at-r)$, and let r be so small that terms involving higher powers than the first may be neglected. Then,

$$F(r-at) = m \sin \frac{\pi}{\lambda}\left\{\phi(at) - \phi'(at)r\right\}$$

$$= m\left\{\sin\frac{\pi\phi(at)}{\lambda} - \frac{\pi r\phi'(at)}{\lambda}\cos\frac{\pi\phi(at)}{\lambda}\right\}$$

$$F'(r-at) = -\frac{\pi m\phi(at)}{\lambda}\cos\frac{\pi\phi(at)}{\lambda}$$

and v or $\dfrac{F'(r-at)}{r} - \dfrac{F(r-at)}{r^2} = \dfrac{m}{r^2}\sin\dfrac{\pi\phi(at)}{\lambda}$.

Now, if the motion of the disturbing surface, instead of being vibratory, be continually increasing or decreasing, λ must be indefinitely great compared to $\pi\phi(at)$ during the whole time of the motion: so that $\sin\dfrac{\pi\phi(at)}{\lambda} = \dfrac{\pi\phi(at)}{\lambda}$, and $v = \dfrac{\pi m\phi(at)}{\lambda r^2} = \dfrac{u}{r^2}\phi(at)$, supposing that $\dfrac{\pi m}{\lambda} = \mu$. At the same time $\dfrac{F'(r-at)}{r} = -\dfrac{\pi m\phi'(at)}{\lambda r}\cos\dfrac{\pi}{\lambda}\phi(at)$ $= -\dfrac{ur}{r^2}\phi'(at)$, which on account of the factor $r\phi'(at)$ is very small compared to v. Hence in this case of disturbance, the part of the velocity accompanied by change of density is very small compared to the whole velocity, and therefore the change of density itself is very small.

Let, for example, $\phi(at)$ be constant; then v varies inversely as r^2, and $-\dfrac{u}{r^2}\cdot r\phi'(at) = 0$, as we should expect. Again, let v, which we may consider to be the arbitrary velocity given to the disturbing surface, be any function of the time, as $f(t)$, and let $r = r'$ when $v = 0$. Then $r = r' + \int f(t)\,dt$, and $\mu\phi(at) = vr^2 = f(t)\left(r' + \int f(t)\,dt\right)^2$. Hence it will be found that

$$-\frac{u}{r}\phi'(at) = -\frac{rf'(t)}{a} - \frac{2v^2}{a}.$$

If v be uniform, $f'(t) = 0$, and $-\dfrac{u}{r}\phi'(at) = -\dfrac{2v^2}{a}$, which is a quantity of an order that has been already neglected. If $v = gt$, $-\dfrac{u}{r}\phi'(at) = -\dfrac{gr}{a} - \dfrac{2v^2}{a}$, in

which if g represent the force of gravity, both the terms are negligible.

Conceive now a small spherical body to descend vertically in the air by the force of gravity. If it be supposed perfectly smooth, it can impress motion on the fluid only in directions perpendicular to its surface. Thus the motion impressed at each instant by the *anterior* half of the sphere is directed from a centre. If v be the velocity of the sphere, $v \cos \theta$ is the velocity impressed in directions making an angle θ with the line of its motion. This case of disturbance is therefore similar to the last, in that the motion is from a centre; but differs in these respects,—the motion is not the same in all directions from the centre, and the centre is not fixed. But I have elsewhere given reasons for concluding (Cambridge Phil. Trans. vol. iii. part 3.) that the equations we have been using, and the results derived from them, apply at each instant to every elementary portion of fluid disturbed in *any* way, provided the condition of the tendency of the motion at each instant, to or from fixed or moveable centres, be fulfilled. If this be admitted, we may at once conclude that the descending sphere is subject to very little change of pressure on its anterior half; for if $\varrho =$ the density of the fluid in contact with any point of it, we find that,

$$a \text{ Nap. log } \varrho = -\frac{r f'(t)}{a} - \frac{2 v^2}{a},$$

in which $f'(t) = \frac{dv}{dt} \cos \theta$, a quantity not very different from $g \cos \theta$, since the resistance of the air is small. The same may be said of the *posterior* half; for it might be shown that the only difference between the disturbances produced by this half and the other, is that the motion is directed *towards* a centre. Similar reasoning is applicable to any kind of increasing or decreasing motion. From all that precedes we draw this conclusion:—

When a small spherical body moves in a medium like air with a velocity small compared to the velocity of propagation in the medium, and in any manner except in rapid vibrations, the pressure on its surface is at every point very little different from the pressure of the medium at rest.

The phænomenon I propose to explain by this result is the spherical form of the drops of rain. That they are spherical is shown by the rainbow. Capillary attraction will account for their assuming in the first instance a spherical form; and from the preceding reasoning it follows that being very small, they do not suffer in passing through the air any inequality

of pressure which will sensibly alter their shape. An inequality of pressure very much less than the weight of a drop would suffice to do this.

Hence also we may account for the success of the common method of making *spherical* shot, by letting them fall in a melted state from a great height, so as to become solid in their descent.

It appears from our reasoning that the resistance to a small spherical body descending in the air, is occasioned very little by the condensation of the air it encounters, but principally by its putting in motion and partly carrying with it a portion of the fluid. Whatever be the law of resistance in regard to the velocity (which it seems difficult to ascertain), we may conceive of the nature of the resistance by supposing a variable mass $mf(v)$ to be always attached to the descending body M, and to be unaffected by gravity; so that if $\mathrm{F} =$ the effective accelerative force, $g\,\mathrm{M} = \mathrm{F}\left(\mathrm{M} + mf(v)\right)$; $\mathrm{F} = \frac{g\,\mathrm{M}}{\mathrm{M} + mf(v)}$.

The foregoing inquiry will also assist us in ascertaining the nature of the resistance of the air to the motion of a pendulum-ball, suspended by a long slender thread. As before, the resistance is not sensibly due to any change of density of the air. The motion being slow and the vibrations of small extent, we may suppose, without chance of sensible error, that the velocity of a particle of the air in the same position relatively to the centre of the ball and the direction of its motion, has always the same ratio to the velocity of the ball. Hence if M be the mass of the ball, μ that of an equal volume of air, m a certain constant, and v the velocity of the ball, we have this equation of *vis viva:*

$$\mathrm{M}v^2 + mv^2 = 2g\,(\mathrm{M} - \mu)\,(h - x),$$

$h - x$ being the vertical descent of the centre of the ball. Hence the vertical accelerative force, or $-\frac{v\,dv}{dx}$, which is the only one that acts, is $g \cdot \frac{\mathrm{M} - \mu}{\mathrm{M} + m}$; and the time of vibration is to the time in a vacuum, as $\sqrt{\frac{\mathrm{M} + m}{\mathrm{M} - \mu}}$ to 1. These results have been obtained by M. Bessel. (*Researches on the Length of the Seconds Pendulum:* Berlin, 1828.)

The last application I propose to make of the preceding analysis, bears upon the nature of light. The undulatory hypothesis of light requires us to give a reason why the planets

meet with no sensible resistance from the ethereal medium. Supposing the æther to be constituted like air, and the matter of the earth and planets to consist of very minute spherical atoms, (suppositions, which I have already advanced to explain some phænomena of light*,) it will follow that no part of the resistance, caused by the condensation of the medium, varies as the simple power of the velocity: also the term $-\frac{r f'(t)}{a}$ must be quite insensible. With respect to that part of the motion of the æther which is unaccompanied by change of density, we may say that the velocity of any particle in the same position relatively to the centre of the planet, and to the direction of its motion, has always the same ratio to the velocity of the planet. The consequence of this would be, that the law of the force tending to the sun would not be changed, but its quantity would be diminished. This effect would be accounted for by taking the mass of the planet of less magnitude than it really is, and therefore probably cannot be easily detected by observations. Hence if any resistance be sensible by any change of the orbits or the periodic times, it will depend on the *square* of the velocity. Also it must be much less sensible in *dense* bodies like the planets, in which the particles that precede diminish the resistance on those that follow, than in such a rare substance as Encke's comet. This singular body has in fact, in the opinion of competent judges, determined the existence of a medium, or something equivalent, resisting according to the square of the velocity.

Papworth St. Everard, April 17, 1832.

XI. *New Experiments relative to the Action of Magnetism on Electro-dynamic Spirals, and a Description of a new Electromotive Battery. By Signor* SALVATORE DAL NEGRO; *with Notes by* MICHAEL FARADAY, *Esq., F.R.S., M.R.I. Corr. Memb. Roy. Acad. Scien. of Paris, &c.*†

[*Addressed to* Dr. Ambrogio Fusinieri, *Director of the Annali delle Scienze, &c. &c.*]

Sir,

ON repeating the experiments relative to the action of terrestrial magnetism on electro-dynamic spirals, an action which was first observed‡ by the two illustrious Italian philo-

* See Phil. Mag. and Annals for March last, p. 161.

† Communicated by Mr. Faraday.

‡ [This is an error. A long section is devoted to terrestrial magneto-electric induction in my original researches (140 to 192) of the date of

sophers Nobili and Antinori, it occurred to me to examine the effect of an ordinary magnet on similar spirals at the moment when one of the poles traversed the axis of the spiral (*Exp. Res.* 39. 41. 114.), and I obtained such results as indicated the path which it would be proper for me to follow, in order to profit by this new property of magnetism. Ultimately I succeeded in constructing a new electrometer, by means of which the efficacy of the instantaneous currents discovered by the celebrated Faraday may be augmented without limit, and obtained in succession with such celerity as to render (as it were) continual the action of these currents*. He [Dr. Fusinieri] has already witnessed the principal part of these my experiments, and more than once has been so good as to assist me faithfully in registering the results, and has solicited a description that might be made public. I did not hesitate to make a brief exposition that might be transmitted and inserted in the forthcoming number of his Journal. He returned from us as quickly as possible, and did not forget to take with him the magnet I had promised.

His most affectionate friend,
Padua, April 20, 1832. SALVATORE DAL NEGRO.

New Experiments, &c. &c.

1. Place a cylindrical tube of paper surrounded by a spiral of silk-covered copper wire upright upon a little table, and connect the extremities of the spiral with a very sensible galvanometer, constructed according to the method of Signor Nobili: introduce the north pole of an ordinary horse-shoe magnet into the axis of the cylinder, and an electric current will be obtained, which will act strongly on the galvanometer. (*Exp. Res.* 39. 147.) On withdrawing the pole of the magnet, a current, in the contrary direction, will be obtained (*Exp. Res.* 39.). On repeating the experiment with the south pole, currents will be manifested in the contrary direction (*Exp. Res.* 114. &c.) to those caused by the north pole, and less powerful, as has been observed.

2. Introduce into the same spiral the north pole of a more powerful magnet than the first, and the conflict will produce a much greater effect; I say, "conflict," because the

December 21, 1831. As my brief letter to M. Hachette is continually taken instead of my memoirs as representing my views of magneto-electricity, I venture to add a few notes and references to this paper, in the same manner as I have done to the paper by Signori Nobili and Antinori, at page 401. of the last volume of the Phil. Mag. and Annals.—M. F.]

[* I have described at length a different but perfect way of obtaining a continuous current by magneto-electric induction. (*Exp. Res.* 90. 154. 155. 156. &c.)—M. F.]

phænomena in question obey the laws of the collisions of solids. The magnetism of rotation discovered by the celebrated Arago has already shown what influence motion has in these phænomena. Then slowly moving the magnet, it may be introduced and removed from the spiral without causing any sensible current. To obtain the maximum effect, it is necessary that the magnetic pole should make its entrance or exit with great velocity. (*Exp. Res.* 136. 153. 258.)

3. Introduce at the same time the poles of the magnet into two equal spirals, having the same direction, and two contrary currents will be obtained, which would destroy each other if the poles of the magnet were of equal strength. But as the north pole is in our latitudes more active than the south, the effect obtained will equal the difference of the two currents, and be in the direction of the greater force; exactly as happens in the collision of solids. It results from this my experiment, that henceforth we may ascertain at once with facility which is the most powerful of two magnets, and how much more active the north pole is than the opposite south pole of the same magnet*.

4. In order to take advantage at the same moment of both the poles of the same magnet, construct two spirals turning in opposite directions, and place them as usual in connection with the galvanometer. Then on introducing the poles of the magnets, an effect will be obtained, equal to the sum of those which could be produced by the poles separately. To measure the effect produced by these two spirals with a more powerful magnet than the first, I was obliged to use a galvanometer of only one-twentieth the sensibility of the first.

5. I immediately perceived that this pair of spirals was a valuable element capable of furnishing a mode of augmenting without limit the efficacy of the instantaneous currents. I therefore instantly constructed a second pair of spirals equal

[* The statement that the north pole is in our latitudes more powerful than the south is a mistake. The cause of the effects obtained by Signor Negro will be found at p. 147 of my *Exp. Research.*, and is dependent on the inductive force of the earth, as a magnet, upon other magnets, as well as upon soft iron. When a straight magnet is held in the dip, or even vertically with its marked pole downwards, both poles are strengthened; when held with its unmarked pole downwards, both poles are weakened. And though when a horse-shoe magnet is held with both poles downwards, as in Signor Negro's experiment, the marked pole is stronger than the unmarked one, it is only because the two limbs are affected as the single magnets just referred to, and the bend of the magnet being the upper part becomes virtually a feeble south pole. If the horse-shoe magnet be held with its poles upwards, then the contrary effect happens, and the unmarked (usually called the south) pole becomes the stronger; or if both poles are in equal relation to the magnetic dip, then both are equally strong.—M.F.]

to the first, and putting both in connection with the galvanometer, I caused two magnets to enter them contemporaneously, and obtained an effect due to the sum of both pair of spirals. On using still more powerful magnets, even the second galvanometer became useless. The galvanometer which I substituted consists of a rhomboidal needle, about five Paris inches in length, and suspended as in the ordinary compass. The wire which connects the extremities of the spirals passes beneath the needle distant about $3\frac{1}{2}$ lines, and is parallel to it when the latter is at rest: on obtaining this fortunate result I conceived the idea of constructing a battery of several magnets put in conflict with an equal number of pairs of spirals.

Construction of a new Electro-motive Battery.

6. I had at command only four magnets, so that for the present I am limited in my construction to four pairs of spirals, as in the manner following: On a little table is placed one after the other four pairs of spirals, with the axes horizontal, and so that the perimeters of the cylinders shall have the same horizontal line as a common tangent, it being parallel to one of the sides of the table. On a second table contiguous to the first, but not in contact, was placed a little carriage consisting of a rectangular table supported on four wheels, by means of which it could easily receive a motion to and fro. The four magnets were placed upon this carriage, so that the poles of each could move horizontally towards the pairs of spirals, and enter within them.

The magnets were firmly fixed on the carriage so as not to alter in position, and the latter was so arranged as to move to and fro only in one direction. On moving the carriage, the limbs of the magnets passed at once into all the spirals, and they could be made to enter or move out with the utmost facility, and with any required velocity.

That the battery thus disposed may give an electric current equal in force to the sum of all the currents excited in the pairs of spirals, it is necessary that all the spirals turning to the right should communicate with each other, that they may form a single metallic wire. The same must be done with all those turning to the left. Then these wires are to be connected in the usual well-known manner with a galvanometer, which we may suppose placed on a third little table, so far distant from the magnets that it may not be influenced by their presence. Although these electric currents are only obtained of instantaneous duration naturally, nevertheless with my battery they may be excited successively with such celerity as to pro-

duce an action, which is as it were continuous*. From the little I have done, and from what I have said, it follows that being able by this method to sum up the simultaneous action of an indefinite number of electric currents, this my battery may become fulminating.

I hope I have said enough to enable my readers to comprehend the mode of constructing this electro-motive battery. Hereafter, and by the help of a figure, I will describe the most useful and convenient distribution of the elementary pairs, and the mode of obtaining the maximum effect when employing the smallest possible number of elements, or of pairs of spirals.

ERRATA relative to Signori Nobili and Antinori's paper. At page 402 of the last Number of Phil. Mag. and Annals, line 23,—for *electromo* read *electrotomo;* and in the corresponding note, for *electronic* read *electrotonic.*

XII. *Account of some Experiments in which an Electric Spark was elicited from a natural Magnet.* By JAMES D. FORBES, *Esq. F.R.S. L. & E. F.G.S.*†

THE recent discovery of Mr. Faraday has conclusively demonstrated, that in every case where a magnetic current is created (to use the word *current* in its ordinary acceptation, as indicative of a peculiar condition, and without reference to any theory whatever), a momentary electric current is induced at right angles to it. The experiment may be shown in two ways: either by mechanically causing a magnetic bar to traverse the axis of a helix of copper-wire of considerable length, —or by causing a piece of soft iron, placed in the axis of such a helix, to connect the poles of a horse-shoe magnet, and thus temporarily acquire polarity.

The second method is that which in my late experiments I have entirely employed; and the subject of them has been a very fine natural magnet, capable of supporting 170 lbs. presented to the University by Dr. Hope. I willingly avail myself of this opportunity to express my obligations to that gentleman for the numerous and important facilities which have been afforded to my researches, in his laboratory, where the magnet still is.

My preliminary experiments demonstrated, by the action

[* See the note at page 46.—M. F.]

† Read before the Royal Society of Edinburgh, April 16, 1832, and abridged from the forthcoming volume of their Transactions: See Phil. Mag. and Annals N. S. vol. xi. p. 359.

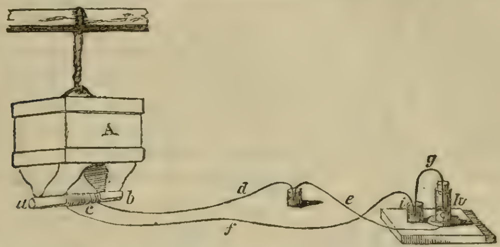
upon the multiplier and upon the frog, that a very powerful and instantaneous current of electricity was conveyed through the helix at the moment of making the contact of the connecting iron with the magnet.

In an early stage of my experiments I had, as far back as the 30th of March, obtained a spark from the magnet, which, however, being unable to repeat, from circumstances of which I afterwards became aware, I did not choose to publish at the time. I accordingly proceeded closely to investigate the circumstances under which sparks were to be obtained from feeble galvanic currents of low intensity. I used the common cylindrical electro-magnetic battery, in which, by varying the charge of acid, I could obtain any required power. Thus I adjusted it till I obtained from a momentary current nearly the same action on the multiplier as I had developed by the magnet. Removing it into a dark place, I found that sparks were obtained at the instant of making and breaking the circuit connecting the cups of the battery. Satisfied that I had a sufficient current of electricity, I proceeded to apply to the magnet the conditions which I had found most effectual for eliciting the spark. These were, 1st, That the spark is more easily obtained at the instant of interrupting than that of completing the galvanic circuit: 2nd, That of the combinations which I tried, a fine pointed iron-wire suddenly withdrawn from contact with a surface of pure mercury, forming part of the circuit, was the most regular in exciting the spark, and that a good deal depended upon the suddenness of the interruption; and, 3rd, That the spark was easiest obtained from the mercury, not at the horizontal upper surface, but where capillary action attracted it to the sides of the containing vessel; and that this was independent of the material of the vessel, being the same with wood, glass, and metal.

I shall now briefly notice the arrangement of the apparatus with which, on the 13th of April, I succeeded in obtaining the spark at pleasure.

The large natural magnet is represented at A. A cylindrical connecter of soft iron *a b*, passing through the axis of the helix *c*, was made to connect the poles of the magnet; accuracy of contact was found to be of considerable importance to the success of the experiment, and one side of the cylinder was carefully formed to a curve of about two inches radius for this purpose. I found great advantage from a mechanical guide, not represented in the figure, to enable an assistant to bring up the connecter rapidly and accurately to the magnet in the dark. The helix *c* consisted of about 150 feet of copper-wire, nearly one-twentieth of an inch in diameter, 7½

inches long, and containing four layers in thickness, which were carefully separated by insulating partitions of cloth and sealing-wax. The one termination *d e* of the wire, passed



into the bottom of a glass tube *h*, half filled with mercury, in which the wire terminated, and the purity of the mercurial surface is of great consequence to the experiment. The other extremity *f* of the helical wire communicated by means of the cup of mercury *i*, with the iron-wire *g*, the fine point of which may be brought by the hand into contact with the surface of the mercury in *h*, and separated from it at the instant when the contact of the connecter *a b* with the poles of the magnet is effected. The spark is produced in the tube *h*.

The success of the experiment clearly depends on the synchronism of the production of the momentary current by connecting the magnetic poles, and the interruption of the galvanic circuit at the surface of the mercury. This might be pretty nearly ensured by a variety of simple mechanical contrivances which suggest themselves,—but as these would require very considerable nicety in their execution, I have been satisfied with the precision which may be insured by a good ear and an accurate assistant,—as I have thus, with a little practice, been able to produce, for many times in succession, at least two sparks from every three successive contacts.

These sparks have generally a fine green colour; that I obtained on the 30th of March was in every respect similar to those I afterwards procured. The intensity of light varies considerably, as it depends on the degree of accuracy with which the circuit is broken at the moment of contact. Sometimes it is highly vivid, and has been seen some yards off in a dark place.

As soon as I had the circumstances under my command, I hastened to show the experiment to my brother, who was present, and to Dr. Gregory, acting secretary of this Society. I afterwards had the satisfaction of showing it to Dr. Hope, to Sir John Leslie, and several other gentlemen.

I beg to repeat, that the success of Signor Nobili's experiment is only known to me through the medium of the public prints; I am quite ignorant of the channel by which the report reached this country; and, at all events, not the slightest clew has been given as to his mode of arriving at the result.

Postscript.—Since the preceding paper was read, and placed in the hands of the printer, I have seen the account of the experiments of Signori Nobili and Antinori, contained in the Number of the *Annales de Chimie et de Physique*, dated December 1831*; and I have likewise, by the kindness of Mr. Faraday, received a copy of his paper about to be published in the Philosophical Transactions. From these documents, it is established, 1st, That Mr. Faraday obtained a spark from a temporary or electro-magnet, as far back as November 1831. This I stated to have been the case in the preceding paper, upon Mr. Faraday's authority, who informed me of it about two months ago; and this was the "*cas particulier*," mentioned in the French version of Mr. Faraday's letter to M. Hatchette, read to the Academy of Sciences, which gave rise to the experiments of Signori Nobili and Antinori, and who also allude to it in their paper, without knowing the real circumstances of the experiment†. It appears, 2ndly, That the first document giving an account of the excitation of a spark by these philosophers, from a *permanent* or *natural* magnet‡, is dated from the Museum at Florence, 31st of January 1832, was published in the *Antologia*, bearing the date of November 1831, and afterwards translated into the *Annales de Chimie*, bearing the date of December. "It is evident," says Mr. Faraday, speaking of the former, "the work could not have been then printed; and though Signor Nobili in his paper has inserted my letter as the text of his experiments, yet the circumstance of the back date has caused many here, who heard of Nobili's experiments by report only, to imagine his results were anterior to, instead of being dependent upon mine§."

The notice of Signor Nobili's experiment, to which I have alluded in my paper as having reached me whilst my investigations were in progress, was that contained in the Literary Gazette for March 24, stating simply the report of the fact, though without naming any authority. I learn from Mr. Fara-

* A translation of the original paper of Signori Nobili and Antinori, with notes by Mr. Faraday, will be found in the Phil. Mag. and Annals, N. S., vol. xi. p. 401.—Edit.

† *Annales de Chimie*, Dec. 1831, pp. 403, 417.

‡ See Mr. Faraday's note, Phil. Mag. and Annals, N. S., vol. xi. p. 405. —Edit.

§ Phil. Trans. for 1832, p. 162, note.

day, that it appeared there by a circuitous channel of information, actually derived from Signor Nobili's communication to himself. The first information I had of Nobili's method of making the experiment, which was in its simplest form almost the same with my own, and explained in terms nearly identical, was not till the *Annales de Chimie* for December reached my hands, which was on the 30th of April, when the foregoing paper was in the press.

Finally, as far as is yet known, no one except Signori Nobili and Antinori and myself have yet obtained the spark from the natural or permanent magnet.

Greenhill, Edinburgh, May 7th, 1832.

XIII. *Remarks on* Mr. White's *Experiments on the Cohesion of Cements; with a tabular View of their Results, reduced to a common Scale. By* B. Bevan, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE papers on *Cements*, communicated by Mr. White, and published in the Phil. Mag. and Annals, N. S., vol. xi. pp. 264 and 333, are of considerable importance, on account of the numerous facts they contain. They enable the architect and builder to know where, and in what manner to apply the different kinds of cement, and the degree of stress which may be safely laid upon them.

A careful perusal of the numerous results will point out several common errors, in respect to the cohesive properties of Roman cement and pozzolano, under different modifications, and under various degrees of exposure to moisture.

And as you probably may be of opinion that an abstract of the results given in these papers, reduced to one common scale in a tabular form, may be acceptable to some of your readers, and save much time to individuals, I take the liberty of sending one.

			Cohesive Strength per square Inch.	
			lbs.	mean.
Cement in bars,	age 6 days,	1 dry	474	356
		2 variable	360	
		3 wet	234	
———	age 47 days,	1 dry	516	450
		2 var.	564	
		3 wet	270	

	Cohesive Strength per square Inch. lbs.	mean.
Cement in bars, age 94 days, 1 dry	210	
2 var.	618	380
3 wet	312	
—— age 187 days, 1 dry	534	
2 var.	708	519
3 wet	336	
Mean of the dry 433		
—— variable ... 562		
—— wet 288		
With salt-water	924	
With 51 per cent. of water	330	
With 64 do.	215	
3 parts cement, 2 parts sand	456	
1 part cement, 1 part brickdust	312	
Bricks.—3 parts cement, 2 parts sand, 6 months	375	
3 —— 2?	362	
All cement ...... 9 months	360	
Paving bricks, best sort	253	
—— seconds	194	
Common building bricks, London*	43	
Common bricks, Soho	412	
Brick cylinders laid in cement	27	
—— in cement and sand	68	
——	48	
——	53	
Brick piers laid in cement, 2 parts; rough lime, 1 part; sand, $1\frac{1}{2}$ part; ...... 1 month.	$4\frac{1}{3}$	
—— pozzolano, 3 parts; Dorking lime, 1 p^t ...... 6 weeks.	7	
—— pure cement	21	
—— pozzolano, 1; stone-lime, 1...	$8\frac{1}{4}$	
—— Atkinson's cement, 1; sand, 1	$25\frac{1}{4}$	
—— Ditto	$49\frac{1}{4}$	
—— cement, 4; lime, 1.	17	

The apparent deficiency of strength in these experiments probably arose from the position of the resultant and strain in being on one side instead of in the middle of the piers?

* Stourbridge fire-bricks have a strength of 790 pounds per square inch. The bricks I used at Greenwich Well were made at Fenny Stratford, and would support 715 pounds per square inch, equal to the strength of Yorkshire stone.

Force required to crush *per square Inch.*

		lbs.
P. 337.	A 14-inch brick pier, laid in cement A	470
	Pozzolano, 3 parts; ground lime, 1...	296
	Atkinson's cement, 1; sand, 1........	410
	Pozzolano, 4; lime, 1..................	638
	Ditto, 3; Dorking lime, 1	600
	Stone-lime, 1; sand, 3..................	500
	Portland-stone pier	2300

A small error may be corrected, Phil. Mag. and Annals, vol. xi. page 339, line 20,—*for* 173½ tons, *read* 149 tons.

Yours truly,

B. Bevan.

P.S.—From the disproportion between the cohesive strength of pure cement, and cement used in brickwork, it is desirable that further experiments should be made on this subject.

XIV. *Addendum to the Paper on a Method for giving the Figures of the Conic Sections to Concave Lenses and Specula, published in No. XII. of the Edinburgh Journal of Science. By* R. Potter, *Esq. Jun.**

HAVING lately had occasion to look into the Transactions of the Cambridge Philosophical Society, I fell upon a paper read before the Society on the 11th December, 1822, by the Rev. W. Cecil, M.A. of Magdalen College, "On an apparatus for grinding telescopic mirrors and object lenses;" where the author, though his principal object appears to be that of describing a machine for superseding manual labour in the general workmanship, shows from mathematical principles the quantity which should be worn away at the various points, to bring a concave lens or speculum to the figure of a conic section. Thus far it appears that I am completely anticipated; but as we differ in our directions for reducing the theory to practice, I submit my claim to the *effectual* invention to the judgement of the scientific world.

Mr. Cecil does not refer to any examination of specula, &c. worked according to his directions. I did not publish my discovery until I had fully proved it, practically, in such large proportions of specula as left no doubt of my procedure being correct. I have prescribed the rotatory effect in the lathe, alone, to be used, *even in the finishing process.* Mr. Cecil prescribes a small lateral motion as well as a circular one, saying, "to grind

* Communicated by the Author.

only by a rotatory motion about the axis would entirely destroy the surface, by producing rings." I maintain that with such a lateral motion no true figure of either an ellipse, a parabola, or an hyperbola, can be obtained; and I have found in my experience no ill result to arise from using, as I prescribe, the rotatory motion entirely, whilst producing the change of figure, even with the inferior dexterity which always accompanies an amateur hand.

XV. *Experiments to determine the Reflection at the second Surface of Flint Glass at Incidences at which no Portion of the Rays passes through the Surface. By* R. Potter, *Esq. Jun.**

THIS subject is one worthy of attention on several accounts. A prism producing total reflection has been proposed as a substitute for the small plane metallic mirror of the Newtonian telescope; and it is generally mentioned in all our optical treatises, that considerable advantage would arise from the substitution, without mention being made of the obvious attendant disadvantages, which would lead to the idea that these latter are trifling compared with the former. This would tend to lead many to make an experiment, bringing some trouble and expense, but in which they could experience no satisfactory result.

In telescopes of large size, where the prism in consequence must be large, and the quantity of glass through which the light passes proportionally so, there would, in addition to the aberration and confusion introduced by the prism, be a disadvantage in point of light also, when the prism became above a certain dimension, depending on the glass which was used. This arises from the property, which all transparent bodies possess, of absorbing a considerable portion of the light which passes through them; and it is greater, even in the most transparent of flint glass, than most persons have any conception of. It will be seen, from the experiments about to be related, that with prisms of only the size which I have employed, the light reflected and transmitted does not greatly surpass that reflected by a mirror of speculum metal which has been properly polished. The experiments furnish also a confirmation of the fact, to which I have drawn attention in a former paper, of the great quantity of light lost in achromatic object-glasses of large size, from the unavoidable thickness of the material through which the light traverses.

I hope this consideration will induce astronomers to inves-

* Communicated by the Author.

tigate the point for themselves; and I feel certain they will be convinced of the superiority of the reflecting telescope for all purposes, excepting perhaps the application to divided instruments; and increased patronage will undoubtedly bring the working opticians to attain a perfection in execution, which there is now so little encouragement to seek for, from the fashionable prepossession in favour of achromatic telescopes.

As a question in physical optics, this subject merits an attention much beyond any which I have been able to give to it, from the difficulty of obtaining a pure glass in sufficient bulk. But though these experiments were only made with prisms of common flint glass, and, as a matter of course, contained many of those waves and striæ to which it is subject, yet they give us results which may be taken as proving the truth of the general opinion,—that no light is lost in what are called total reflections in transparent bodies; and we should consequently conclude that it is the same for all incidences at which this effect takes place.

If there is *any* variation in the intensity of the reflections, it is evidently very small, and much more perfect prisms and longer attention would be necessary to determine it. It is on account of the imperfection in the glass that I have not so multiplied the experiments as would otherwise have been desirable, knowing that no decisive argument could be drawn from them where the differences to be detected, if any, were evidently so very small; and the experiments in photometry with lamps present nothing very enticing and pleasant in themselves, and require, besides, considerable practice and patience to get uniformly very exact results.

The only correct method of proceeding in this inquiry is to have the prisms formed so that the light may be incident and emergent perpendicularly to the surfaces, and falling at the required angle on the surface producing total reflection; and also, which is of equal importance, that the thickness of glass through which the rays pass may be the same in all. The prisms I have used were similar in shape to fig. 1. 2. and 3, where the distances *a b*, *b c*, are equal in all; and by a rectangular piece similar to fig. 4, where the length *a c* is equal to the sum of the two lengths *a b*, *b c* in the prisms, we learn the quantity of light transmitted under all similar circumstances, excepting the total reflection, which enables us to complete our deductions, by allowing for the loss attending a direct transmission.

The length *a c*, or the sum of the lengths *a b*, *b c*, was 1·98 inch, and the other dimensions of the sections of the prisms were proportionally as represented in the figures, the depths of each being equal to the depth *a e* of the rectangular pieces.

The prism fig. 1. was cracked in the commencement of the experiments, by being placed too near the flame of the lamp; but fortunately the light in this prism being incident at 45° on the second surface, it was allowable to use another part of the glass beyond the extent of the crack.

The rectangular piece

averaged from	rays incident.	rays transmitted.
3 measurements	. 100	. . . . 78·56
4 ———	. 100	. . . . 78·36
5 ———	. 100	. . . . 77·95

The prism fig. 1, where the light was incident on the second surface at an angle of 45°,

averaged from	rays incident.	rays reflected and transmitted.
8 measurements	. 100	. . . . 76·97
6 ———	. 100	. . . . 75·23

The prism fig. 2, where the light was incident on the second surface at an angle of 60°,

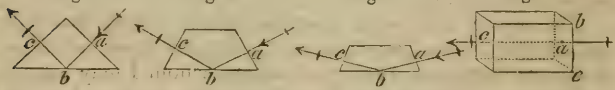
averaged from	rays incident.	rays reflected and transmitted.
6 measurements	. 100	. . . . 74·97
6 ———	. 100	. . . . 78·46

The prism fig. 3, in which the light was incident on the second surface at an angle of 75°,

averaged from	rays incident.	rays reflected and transmitted.
6 measurements	. 100	. . . . 77·04
5 ———	. 100	. . . . 76·90

Another set of measurements for the second prism gave 72·92; and thinking that there arose rather more of extraneous light in using the rectangular piece than in using the prisms, I made another set of measurements with it, taking additional precautions, and obtained 74·02; but it is probable that in these two last-mentioned cases some unnoticed cause of error had arisen, making the results come out too small.

Fig. 1. Fig. 2. Fig. 3. Fig. 4.



XVI. *On the Action of Light in determining the Precipitation of Muriate of Platinum by Lime-water; being an Extract from a Letter of Sir* John F. W. Herschel, *K.H. F.R.S. &c. to Dr.* Daubeny*.

WHEN a solution of platinum in nitro-muriatic acid, in which the excess of acid has been neutralized by the

* Read before the British Association at Oxford, June 22, 1832; and communicated by request of the Author.

addition of lime, and which has been well cleared by filtration, is mixed with lime-water, in the dark, no precipitation to any considerable extent takes place,—for a long while indeed, none whatever; though after very long standing, a slight flocky sediment is formed, after which the action is arrested entirely. But if the mixture, either freshly made, or when cleared by subsidence of this sediment, is exposed to sunshine, it instantly becomes milky, and a copious formation of a white precipitate (or a pale yellow one if the platinic solution be in excess,) takes place, which subsides quickly, and is easily collected. The same takes place more slowly in cloudy daylight.

This remarkable action is confined to the violet end of the spectrum. I have exposed tubes of the mixed liquids immersed in the sulphuric tincture of red rose-leaves, to strong sunshine for whole days, and (after the first slight deposit already mentioned, which ceases in the first hour,) the remainder is altogether insensible to red light; but the moment it is taken out of the red liquor and held in free sunshine, the usual precipitation takes place as copiously as if it had been all the time kept in total darkness. Even yellow liquids suffice to defend it.

The precipitate itself is a remarkable one, being a combination of the oxide of platinum with lime, in which the oxide seems to perform the part of an acid (a property of this oxide which I believe has been before remarked; though at this distance from my books I cannot say by whom). Muriatic acid dissolves it readily without effecting any decomposition, even when added in too small quantity to take up the whole. Nitric acid also dissolves it; (when newly formed and moist, entirely; when dried, with some residue of oxide). The nitric solution is precipitated by nitrate of silver, and the precipitate, which is of a high orange colour, and which is a true *platinate of silver*, is easily distinguished from muriate of silver, not only by its colour, but by its insolubility in the liquid hyposulphites.

The above facts were observed by me nearly two years ago, and have been shown by me to a great many individuals at various times in the interval; among whom I may mention the Bishop of Cloyne and Dr. Somerville, in June last (if I recollect right); Sir D. Brewster, Mr. Babbage, Mr. Talbot, and others, in London, last summer, and more recently to yourself; and have been distinctly described to many of my scientific friends in conversation, among whom I will only particularize Mr. Ritchie. I mention these circumstances merely as ascertaining my *early* and *independent* observation of a fact which, at the time of its discovery, I considered to be *sui generis*, and which I cannot regard as of slight import-

ance either in a photological or a chemical point of view*. My only reason for not at once making it public, was a desire to satisfy myself as to the real distinction (if any) between the white and yellow precipitate formed, when the proportion of lime-water to the platiniferous solution is in excess, and in defect, as also to ascertain the nature of that sedimentary deposit which is formed *independently* of the action of light. With a view to this inquiry, I have now in preparation (as you have seen in my laboratory,) a considerable quantity of the several precipitates in question.

Hamburg, June 12, 1832. J. F. W. Herschel.

XVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

March 15.—A PAPER was read, entitled "Further Notice of the new Volcano in the Mediterranean." By John Davy, M.D. F.R.S. Assistant Inspector of Army Hospitals.

The author states that since the 25th of October, the date of his last communication to the Society, the crater of the volcano has undergone several changes of form, and has now entirely disappeared. He infers from the phænomena observed, that the crater was one of *eruption*, composed entirely of loose materials, thrown up by volcanic action, and not one of *elevation*, that is, formed of rock which once composed the bed of the sea. In July the heat at Malta was very close and oppressive, the thermometer rising more than once to 105° of Fahrenheit, and the western sky had a dark lurid red hue: but these atmospheric states are regarded by the author as independent of the volcano, for the temperature of the air in its immediate vicinity was very little affected by it.

A Paper was also read, entitled "A method of deducing the Longitude from the Moon's Right Ascension." By Thos. Kerigan, R.N. Communicated by Admiral Sir Edward Codrington, F.R.S.

The author has recourse to the moon's right ascension as an element for determining the true meridian of the place of observation: his method being an extension of that given by him in the first volume of his "Mathematical and General Navigation Tables." He gives examples of the application of this method, and considers that with the aid of a chronometer showing the approximate mean time at Greenwich, the longitude of any given place may be determined, either at sea or on land, within very narrow limits of error, and with much greater practical convenience than by the ordinary method of lunar distances.

March 22.—The reading of a Paper, entitled "An Account of some experiments and observations on the Torpedo," by John Davy, M.D. F.R.S. Assistant Inspector of Army Hospitals, was commenced.

* It may be proper to mention that these remarks are made by Sir John Herschel, with reference to the article headed "Chemical Action of Light," &c. given, from the *Journal de Pharmacie*, in the last Number of the Phil. Mag. and Annals, p. 466.—Edit.

March 29.—A Report, drawn up by the Rev. William Whewell, M.A. F.R.S., and John William Lubbock, Esq. M.A. V.P. and Treas. R.S., on Professor Airy's Paper, read before the Royal Society on November 24, 1831, and entitled, "On an Inequality of Long Period in the Motions of the Earth and Venus," was read.

The conclusion of this Report is as follows:

"We regard this paper as the first specific improvement in the solar tables made by an Englishman since the time of Halley, as valuable from the care which the author has employed in the numerical calculations, as well as for the sagacity he has displayed in the detection of an inequality so small, and of so large period; and we recommend its insertion in the Philosophical Transactions."

A notice of Prof. Airy's Paper will be found in the Phil. Mag. and Annals, vol. xi. p. 117.

April 5.—The following Report, drawn up by Samuel Hunter Christie, Esq., M.A. F.R.S., and John Bostock, M.D. V.P.R.S., on Mr. Faraday's Paper, read before the Royal Society on December 15, 1831, and entitled "Experimental Researches in Electricity," was read*.

Report.

In the first section of this paper, the author considers the induction of electricity in motion.

Shortly after the discovery by Oersted of the influence of electricity in motion on a magnetic needle, it was almost simultaneously discovered by Arago, Davy, and Seebeck, that iron became magnetic by induction from the connecting wire of a voltaic battery, or the passage of an electric current; but though the effects at first observed were afterwards greatly increased by peculiar arrangements, induction was in all cases restricted to iron. Arago's beautiful experiments on magnetic needles vibrating within metallic rings, and on the mutual action of all metals and magnets, when either is in motion, are undoubtedly instances of a peculiar magnetic induction in other metals than iron; but the very doubtful experiment of Ampère can scarcely be adduced as one. The singular results obtained by MM. Marianini, De la Rive, and Von Beek, referred to by our author, are probably due to electric induction. But none of these can be considered as having originated the discoveries described in the present paper, excepting so far as all new views originate in the contemplation of results previously obtained.

In this section of his paper the author shows that a peculiar state is induced in a copper wire which is in the immediate neighbourhood of another, through which an electric current passes, that is, which forms the connecting wire in a voltaic circuit. This state of the wire was manifested by its action on a magnetised needle, and by the induction of magnetism in steel wire submitted to its action.

Two copper wires, each more than 200 feet in length, were wound in the same direction round a large block of wood, the coils of the

* See Phil. Mag. and Annals, N.S. vol. xi. pp. 300, 401, 462, 465; and the present Number, pp. 45, 49, & 76.

one being interposed between those of the other, and metallic contact everywhere prevented. The ends of one wire were connected with a galvanometer, and with the ends of the other, contact could be made or broken with a battery of one hundred and twenty pairs of plates. On the contact with the battery being made, the needle of the galvanometer was invariably impelled in one direction, and on the interruption of the contact, it was always impelled in the contrary. After the first impulse on the completion of the voltaic circuit, the needle resumed its natural position, no permanent deflection whatever occurring during the time that this circuit remained complete.

On substituting a helix of copper wire formed round a glass tube for the galvanometer; introducing a steel needle; making contact, as before, between the battery and the inducing wire; and then withdrawing the needle, previously to breaking the battery contact, it was found to be magnetised. If the contact was first made; a needle introduced in the tube; the contact broken; the needle on being withdrawn was found to be magnetised to the same degree nearly as the first, but the poles at the corresponding ends were of the contrary kind.

If the circuit between the wire under induction and the galvanometer was not complete when the contact with the battery was made, then no effect on the needle was observable either on completing or again breaking the first circuit. But the battery communication being *first* made, and *then* the wire under induction connected with the helix containing the needle, on interrupting the battery circuit, the needle was magnetised. These last facts, in a theoretical point of view, are most important: they prove that on completion of the voltaic circuit, the state of the wire under induction undergoes a double change, the one momentary, the other permanent so long as the voltaic circuit remains complete, and only exhibiting a momentary action on the interruption of that circuit.

From the experiments detailed in this section, the author concludes, that currents of voltaic electricity produce, by induction, currents (but which are only momentary) parallel to or tending to parallelism with the inducing currents; that the induced current, by the first action of the inducing current, is in the contrary direction to, and by its cessation in the same direction as, that of the inducing current.

The author next introduced iron into his arrangement, by which means a double induction took place, the iron itself becoming magnetic by induction, in the first instance, and electricity being induced in the copper wire from the magnetised iron, in the second. The effects were here of precisely the same character as before, but greatly increased. By this arrangement unequivocal evidence of electricity in the wire under induction was obtained; for not only was the needle in the galvanometer violently affected, but a minute spark could be perceived on using charcoal at the ends of that wire.

On dispensing altogether with the voltaic arrangement, and substituting for the electro-magnet a cylinder of soft iron, rendered

magnetic by contact with two bar magnets, or a common cylindrical magnet of steel, similar results were still obtained. The arrangement and the effects were simply these: several helices of copper wire were formed, in the same direction, round a hollow cylinder of pasteboard, metallic contact being prevented between the contiguous coils: of these, either the *alternate ends* were united, to form *one* long helix, or *all* the corresponding ends to form a *compound* helix; and within the pasteboard cylinder, a cylinder of soft iron was introduced: on the ends of this cylinder being brought into contact with the poles of two bar magnets, united at the other ends so as to resemble a horse-shoe magnet, the needle of the galvanometer was impelled in one direction, and on the contact being broken, in the contrary. Similar effects were produced by simply introducing a cylindrical steel magnet into the hollow cylinder over which the copper wire was wound. The effects were strikingly increased, but were still of precisely the same character, when Knight's large compound magnet, belonging to the Royal Society, was substituted for the bar magnets. Here, the mere approximation to the magnet, of the compound helix, whether containing the cylinder of soft iron or not, was sufficient to impel the needle in one direction, and its recess from the magnet, to give a contrary impulse. But even here, the effects were purely impulsive, the needle invariably returning to its undisturbed direction, when the contact was continued.

As in the voltaic arrangement, a small voltaic apparatus, sufficient to deflect the needle of the galvanometer 30° or 40°, being introduced between the galvanometer and the helix under induction, produced no effect on the impulses given to the needle, on making and breaking contact of the iron cylinder with the magnet: nor did the power of this arrangement appear to be affected after making the contact or after breaking it.

Although all attempts to obtain chemical effects or a spark in this case failed, yet we agree with the author that these experiments prove the production of electricity by ordinary magnetism, and think the reasons which he adduces for its want of energy satisfactory *.

This discovery has therefore supplied the link in the chain of connexion between electricity and magnetism, which has been wanting since Oersted's discovery. That the electricity developed acts in a peculiar manner, so far from diminishing the interest attached to the discovery, adds greatly to its value.

After the detail of these perfectly original and highly interesting experiments, the author considers the peculiar electric state of the wire when subjected either to volta-electric or magneto-electric induction. This state he terms the electro-tonic state.

Unlike the induction from electricity of tension or the ordinary

* Since this report was written, a brilliant electric spark has been obtained by Mr. Faraday and Mr. Christie with this magnet, by the very means which, at this time, failed, in consequence of two contacts not taking place at the same instant, on which circumstance the success of the experiment appears entirely to depend.

induction from a magnet, this state of the wire is not analogous to that of the inducing wire; for whatever may be the permanent state of the wire under induction while the voltaic circuit is complete, or the magnetic contact is unbroken, so long as either of these continues, there is no evidence of any change having taken place in it, and its change of state is only rendered manifest at the instant of interrupting the circuit or the contact, and at that of again renewing them; impulsive forces being brought into action at either instant, but in contrary directions in the two cases.

The author observes, that this peculiar condition shows no known electrical effects whilst it continues, nor has he yet been able to discover any peculiar powers possessed by matter whilst retained in this state; that no re-action is shown by attractive or repulsive powers; that no retarding or accelerating power is exerted upon electric currents passing through metal in the electro-tonic state, that is, the conducting power is not altered by it; that all metals take on this peculiar state; that the electro-tonic state is altogether the effect of the induction excited, and ceases with the inductive power; that this state appears to be *instantly* assumed, the force brought into action at the instant of its assumption being merely impulsive.

The author considers that the current of electricity which induces the electro-tonic state in a neighbouring wire, probably induces that state also in its own wire, and that this may be the case with fluids and all other conductors; and concludes that if it be so, it must influence voltaic decomposition and the transference of the elements to the poles. Should facts be found to accord with these views, we consider the author fully justified in his anticipations of the importance of his discovery as applicable to the decomposition of matter, and we certainly feel that the discovery could not have been made by any one more likely to decide this question, or more able to avail himself of a new principle of decomposition when discovered.

In the series of actions proceeding from the voltaic battery which this discovery exhibits to us, a very curious succession is observable. Volta-electricity passes along the connecting wire of the battery, electro-magnetism at right angles to it. By this means the cylinder of soft iron, within the helix into which the connecting wire is formed, becomes a magnet. If the poles of the magnet be joined by an iron bar, ordinary magnetism passes along this bar, but magneto-electricity is induced at right angles to it in a helix wound round it. And again, magneto-electricity is propelled along the wire, and magnetism is induced in a steel bar at right angles. This bar may again induce magneto-electricity in a wire at right angles to it, by which another bar may become magnetic; and so on, showing a repetition of similar powers successively brought into action, but their efficiency at each step greatly diminished.

The effects hitherto described were due to a momentary action: in order to obtain continuous action the author applied the principle of circular motion. For this purpose a thick copper disc was made to revolve near the magnet, so that a portion near its edge passed between the ends of two bars of iron which concentrated and ap-

proximated the poles. The edge and a portion round the centre of the disc were well amalgamated: an amalgamated conductor was applied to the edge of the disc near the poles, and with this, one end of the wire of the galvanometer was connected, the other end being connected with the centre of the disc. While the disc revolved, the needle of the galvanometer was permanently deflected at least 45° in one direction; and when the motion of the disc was reversed, the permanent deflection was in the opposite direction.

When the disc revolved horizontally in the direction of the sun's daily motion, the unmarked pole being beneath the disc and the marked pole above, it appeared, by the indications of the galvanometer, that positive electricity was collected at the edge of the disc nearest to the poles: if the marked pole was below and the unmarked pole above, then negative electricity was collected at that part of the disc: and if in either case the direction of the motion was reversed, the nature of the electricity collected at the same place was also reversed.

The experiment being made in a still more simple form, by passing a plate of copper longitudinally between the poles of the magnet, it appeared that positive electricity was collected on one edge of the plate, and negative on the opposite; and if the plate was passed in the contrary direction, then the electricities on the edges were reversed.

When a wire was passed laterally between the poles, similar results were obtained.

The law according to which the electricity excited depends upon the pole of the magnet near which a wire moves, and the direction of its motion, although not so expressed by the author, appears to be this: Let the wire revolve parallel to itself about a bar magnet, so that its centre coincides with any curve;—for example, (in order to mark more readily the points where the direction of the current of electricity changes,) with an ellipse, the major axis of which coincides with the axis of the magnet, and the minor axis passes through its centre; let the wire be inclined at any angle to the plane of the ellipse, which in the first instance we will suppose to be horizontal, and that the marked end of the magnet is pointing north; and let the wire move parallel to itself in the direction of the sun's daily motion; then while the wire revolves from the *western* extremity of the axis minor round the *marked* pole to the *eastern* extremity, the electric current will be from the end of the wire *below* to the end *above* the orbit: while it is revolving from the *eastern* extremity round the *unmarked* pole to the *western* extremity of the axis minor, the current of electricity will be from the upper to the lower end of the wire; and whatever position the plane in which the wire revolves may take by revolving about the axis of the magnet, or whatever may be the position of this axis, still the current of electricity will be from the end of the wire in the same position, relatively to the plane of revolution, as before. If the direction of the motion be reversed, the direction of the current will likewise be reversed.

It would follow from this, that if two wires parallel to each other,

on opposite sides of a bar magnet, and perpendicular to its axis, be moved along the sides of the magnet in the same direction, the currents of electricity in them will be in opposite directions; and hence we may draw this important conclusion,—that there must be some internal arrangement in a magnet, whether of currents or of particles, which renders the same absolute motion, a motion in contrary directions relatively to such arrangement on the opposite sides of the magnet.

From all these experiments the author concludes, that when a piece of metal (and the same may be true of all conducting matter,) is passed either before a single pole, or between the opposite poles of a magnet, electric currents are produced across the metal, transverse to the direction of motion; and which therefore in M. Arago's experiments approximate towards the direction of radii. Assuming the existence of these currents, he satisfactorily accounts for the phænomena observed in these experiments and in those by Mr. Babbage and Sir John Herschel. Thus, the disc revolving in the direction of the sun's daily motion beneath the marked pole of a magnet, currents of positive electricity set from the central part towards the circumference near the pole, and the action of these currents is to move the pole also in the direction of the sun's motion; so that the magnet, if at liberty to revolve, will move in the same direction as the disc.

Electric currents similar to those produced by passing copper between the magnetic poles, were produced by iron, zinc, tin, lead, mercury, and all the metals tried. The carbon deposited in the coal-gas retorts also produced the current, but ordinary charcoal did not; nor could any sensible effects be produced with brine, sulphuric acid, or saline solutions. Although the author succeeded in obtaining a continuous current of electricity by means of the revolving disc, yet he was not able, by this means, to produce any sensation upon the tongue, to heat fine platina wire, to produce a spark with charcoal, to convulse the limbs of a frog, or to produce any chemical effects. That he should have failed in obtaining these most striking effects of electricity, we attribute to the feebleness of the electricity excited, and feel assured that by adopting means greatly to increase the intensity, all these effects will result from the electricity derived from ordinary magnetism.

The facts contained in this paper of Mr. Faraday's, and the conclusions which he draws from them are so important, that we feel we should not have done justice to the communication, had we not given an abstract of the whole, at the same time that we stated our opinion of its value. Had the author's discovery consisted alone of the simple fact, that steel may be magnetised by a distant magnet, in a manner similar to that employed with the voltaic battery, we should have considered it of the highest importance in the inquiry concerning the connexion between magnetism and electricity; but when we see permanent effects which, hitherto, have only been derived from electricity, now derived from the common magnet, by calling in the aid of motion, showing clearly that electricity can thus be excited;

and find that the laws which govern the phænomena are established, we cannot but entertain hopes that a door has been opened through which may at length be discovered the precise distinction between two agents which in many respects so greatly resemble each other in their effects and in their laws of acting. Such being our opinion of the results obtained by Mr. Faraday, we can have no hesitation in recommending most strongly the publication of his paper in the Transactions of the Royal Society.

(Signed) S. H. Christie.
J. Bostock.

Dr. Davy's Paper on the Torpedo, was then read in continuation.

April 12.—The reading of Dr. Davy's Paper, entitled, "An Account of some experiments and observations on the Torpedo," was resumed and concluded.

The late Sir Humphry Davy gave an account, in a paper published in the Philosophical Transactions for 1829*, of some experiments which he made on the Torpedo, with the view of ascertaining how far its electricity is analogous to that of the voltaic, or other galvanic batteries; but the results he obtained were altogether of a negative kind. He was prevented by the declining state of his health from prosecuting this inquiry, which he was still ardently bent upon completing, and which he requested his brother would carry on after his death. The author, accordingly, when at Malta, being in a favourable situation for obtaining living torpedos, made the series of experiments which are related in the present paper. They entirely confirm those of Mr. Walsh made in 1772, and which established the resemblance of the agency exerted by this fish to common electricity; and they also prove that, like voltaic electricity, it has the power of giving magnetic polarity to steel, of deflecting the magnetic needle, and also of effecting certain chemical changes in fluids subjected to its action. Needles perfectly free from magnetism were introduced within a spiral coil of copper wire, containing about 180 convolutions; the whole coil being an inch and a half long and one tenth of an inch in diameter, weighing only four grains and a half, and being contained in a glass tube just large enough to receive it. On the electric discharges from a vigorous torpedo being made to pass through the wire during a few minutes, the needles were rendered strongly magnetic. The same influence transmitted through the wires of the multiplier produced very decided deflexion of the needle; the under surface of the electrical organ of the torpedo corresponding in its effect to the zinc plate of the simple voltaic circle, and the upper surface corresponding to the copper plate. No effect of ignition could be perceived when the discharge from the torpedo was made to pass through a silver wire one thousandth of an inch in diameter: nor could unequivocal evidence be obtained of the production of sparks on interrupting the circuit; the slight luminous appearances which occurred being probably of the same kind

* Sir H. Davy's Paper on the Torpedo will be found in Phil. Mag. and Annals, vol. vi. p. 81.

as those often exhibited by sea water when agitated. A small gold chain, however, composed of sixty double links, was found to be capable of transmitting the shock; a fact which seems to show that air is not impermeable to the electricity of the torpedo. When fine silver wires, interrupted by a solution of common salt, were placed in the circuit, minute bubbles of air collected round the point communicating with the under side of the torpedo, but none at the other point. When gold wires, instead of the silver ones, were used, gas was evolved from each of the extremities; but in greatest quantity, and in smaller bubbles, from the lower, than from the upper wire. With a strong solution of nitrate of silver, the point of the lower gold wire became black, and only two or three bubbles arose from it; the point of the upper gold wire remaining bright, and being surrounded with many bubbles. Similar, but less distinct, results were obtained by employing a strong solution of superacetate of lead.

The remainder of the paper is occupied with a detailed account of the anatomical structure of the electrical organs of the torpedo, and of the muscles that surround them. The texture of the columnar portions of those organs appears to be homogeneous, with the exception of a few fibres, probably branches of nerves, which pass into them. A large quantity of water, separable by evaporation, enters into their composition: and they undergo spontaneous changes more slowly than the muscles. They are incapable of contraction by any of the ordinary stimuli, and even that of an electric shock from a voltaic battery, applied either to the organs themselves or to the nerves which supply them. Hence the conclusion is drawn that these organs are not muscular, but that their columns are formed by tendinous and nervous fibres, distended by a thin gelatinous fluid.

The anatomical account is concluded by a description of the origin, course, and distribution of the nerves belonging to the electrical organs. The author found that the gastric nerves are derived from these; and hazards the conjecture that superfluous electricity may, when not required for the defence of the animal, be directed to the stomach, so as to promote digestion: in corroboration of which he cites the instance of a torpedo which, when living, had been frequently excited to give shocks, and in whom a small fish found in its stomach after death, appeared to be totally undigested. The secretion of mucus was also either suppressed or considerably diminished. From the circumstance that the branchiæ are supplied with twigs of the electrical nerves, the author conceives there may be some connexion between the electrical and the respiratory functions; and that the evolved electricity may be employed in decomposing water, and in thus supplying the system with air, in situations where the animal has not access to that of the atmosphere. The author considers the mucous system of the torpedo as performing important offices in its œconomy, in consequence of its connexions with the electrical nerves. Contrary to the statement of Mr. Hunter, he finds that the electrical organs are very scantily supplied with blood-vessels. He concludes by some remarks on the peculiar

characters of the electricity of the Torpedo, the purposes it appears to serve, and the varieties exhibited by different individuals, according to the age, the sex, and other circumstances.

The Meetings of the Society were then adjourned over Easter to the third of May.

May 3.—The following Report, drawn up by the Rev. William Whewell, M.A. F.R.S., the Rev. George Peacock, M.A. F.R.S., and the Rev. Henry Coddington, M.A. F.R.S., on Mr. Lubbock's Paper, read before the Royal Society Feb. 9, 1832, and entitled, "Researches in Physical Astronomy,"—was read.

Report.

The method of the variation of parameters as applied to the investigation of the perturbations of the solar system has been successively developed in modern times. This method gives the variations of the elements of the elliptical orbit in terms of the differentials of a certain function R of these elements, and of the disturbing forces. Euler, Lagrange (1783), Lagrange and Laplace (1808) obtained the formulæ for $d\,\alpha$, $d\,e$, $d\,\varpi$, $d\,p$, $d\,q$ where $p = \tan\phi \sin\theta$, $q = \tan\phi \sin\theta$. Poisson first gave the expression for $d\,\varepsilon$. Pontécoulant, p. 330, has introduced $d\,\iota$ and $d\,\nu$ instead of $d\,p$ and $d\,q$; but those developments gave expressions neglecting the square of the disturbing force. Mr. Lubbock has published (in a Paper in the Phil. Trans. April 1830,) expressions which include the effect of any power of the disturbing force. This method has been principally applied to the secular inequalities; but it is susceptible of being applied with no less strictness to periodical inequalities, all of which may be represented by certain changes in the elements of the elliptical orbit.

But the same problems may also be approximately solved directly; for we obtain a differential equation involving the radius vector and the time. In this equation there occurs the same function R of which we have already spoken; and this function is expanded according to terms involving cosines of the mean motions of the disturbing and disturbed planet, and cosines of the difference of certain multiples of these motions. This expression has been treated of by various authors, and among others Mr. Lubbock has himself (in memoirs read May 19 and June 9, 1831,) given the expansion of R in a form suited to his present object.

The coefficients of the terms in this expansion are arranged, as usual, according to the order of the excentricities, their powers and products, and to the power of the $\sin^2$ of half the inclination. These coefficients involve also certain quantities $b_{n,i}$ where n and i have a variety of values; and these quantities depend on the ratio of the mean distances of the disturbing and disturbed bodies from the sun.

Solving the differential equation which involves r, by the equating of coefficients, Mr. Lubbock finds a value for the reciprocal of r in such terms as have been mentioned. By certain algebraical transformations of the fractional coefficients in which i occurs, (and by certain equations of condition between $b_{3,i-1}$, $b_{3,i}$, b_{3i+1}, and between similar quantities,) the expression for the reciprocal of r is transformed and reduced, the arcs remaining as they were.

But by the properties of the ellipse, the reciprocal of r is equal to a series of terms involving the excentricities, and involving also cosines of the mean anomaly and its multiples: and hence the variation of this reciprocal is equal to a similar series, involving sines and cosines of such arcs, and involving also the variations of the elliptic elements. By substituting the variations of the elliptic elements given by the formulæ above mentioned, when we put for R its expansion, we have a certain series of sines and cosines with their coefficients multiplied into certain other sines of the same kind.

It is found that the sines and cosines thus multiplied produce, by trigonometrical transformations, arcs identical with those which were found in the value of the reciprocal of r obtained by the former method; and the coefficients are also found to be identical with those resulting from the former transformations and reductions.

We have not thought it necessary to verify the somewhat complex reductions by which Mr. Lubbock has shown the identity of the results obtained by these two methods. The mode of proceeding is perfectly satisfactory, and the truth of the conclusion might have been foreseen. The reductions, however, by which identity was to be exhibited were by no means obvious: and we conceive it not unlikely that the development of them may sometimes be of use in enabling us to judge which of the two methods of solution may be applied with most convenience in particular cases.

We are of opinion that this Paper is well worthy of being printed in our Transactions. (Signed) W. Whewell.
Geo. Peacock.
H. Coddington.

LINNÆAN SOCIETY.

June 5.—A paper was read, entitled, "Additional Observations on the Sexual Organs and Mode of Impregnation in *Orchideæ* and *Asclepiadeæ*," by Robert Brown, Esq. V.P.L.S.

These additional observations to a paper communicated to the Society in November, and of which an abstract is given in the Phil. Mag. and Annals for December last, relate entirely to *Orchideæ*.

The author begins by remarking, that as the tubes forming the mucous cords were never observed in the cavity of the ovarium until after the application of the pollen to the stigma, and as these tubes very nearly resemble those immediately derived from the pollen, he had in the paper referred to considered them as having the same origin.

But as he has since ascertained in several cases, especially in *Bonatea speciosa*, that the application of a small portion of a pollen-mass to the stigma is sufficient for the production of mucous cords of the usual size, and as the number of tubes thus produced is much greater than that of the grains of pollen actually applied, he is now led to believe, that these tubes do not immediately proceed from the pollen, though its application to the stigma is necessary for their production. In what manner they are generated, however, he does not attempt to explain. He finds in *Bonatea*

speciosa, as well as in the other *Orchideæ* examined with this view, that the earliest appearance of these tubes is in the tissue of the stigma in the immediate neighbourhood of the pollen tubes, from which they are with difficulty distinguishable, and only by their generally more flattened and less granular appearance, and by those interruptions in their supposed cavity which he had formerly observed, and termed coagula;—that from this part of the tissue they gradually descend, at the same time increasing apparently both in number and length, until they arrive in the cavity of the ovarium, where the cords which they form also by degrees elongate and subdivide in the manner described in the original paper. In addition to the account there given, he observes that although in several cases he has not been able to trace any tubes going off from the six principal cords, yet that in others, and particularly in *Orchis Morio*, he has seen them scattered over the whole surface of the placentæ; and in the same species, in several, though not in many instances, he has been able to trace a single tube to the aperture of the testa of an ovulum. Since this paper was read, the author has found in *Habenaria viridis*, in like manner, and in many cases, tubes inserted into the apertures of ovula.

To account for the greater part of the flowers of an Orchideous spike being fecundated, which not unfrequently happens, he observes that from the greater degree of viscidity existing in the retinaculum than in the stigma, and from the viscidity of the surface of this organ being sufficient to overcome the mutual cohesion of the lobules of pollen in most *Ophrydeæ*, a single insect may readily impregnate many flowers with one and the same mass of pollen; a fact which he has confirmed by experiment in *Bonatea*. He observes, however, that even in *Ophrydeæ* exceptions occur to these relative degrees of viscidity, especially in *Ophrys*, the insect forms of whose flowers are so striking; and as he finds, that in this genus the assistance of insects in impregnation is less necessary, he concludes that these forms are intended rather to repel than attract, and he adds that the flowers of *Orchideæ* having those remarkable forms, resemble the insects of the country in which the plants are found.

And lastly, he remarks, that in a few cases, from the relative position of the parts of the flower, the pollen-masses are brought into contact with the secreting surface of the lateral stigmata, and the assistance of insects is therefore wholly superseded, as in *Neottia elata*, which, accordingly, seldom fails uniformly to ripen its capsules.

The East India Company have presented to the Linnæan Society their magnificent Herbarium, containing the plants collected between long. 73° to 114° E. and lat. 32° N. to the equator, by König, Roxburgh, Rüttler, Russell, Klein, Hamilton, Heyne, Wight, Finlayson, and Wallich. It includes about 1300 genera, more than 8000 species, and amounts, in duplicates, to at least 70,000 specimens,—the labours of half a century.

For many years a large portion of these vegetable riches were stored on the shelves of the India House, without any one sufficiently conversant in Indian Botany to arrange and render them subservient to the cause of science. On the arrival in this country of Dr. Wallich, the distinguished superintendant of the Company's Garden at Calcutta, in the year 1828,—who brought with him an immense accession to the Herbarium from various parts of India, especially Nipal and the Burmese Empire,—the Court of Directors instructed him to make a Catalogue of the aggregate collection, and to distribute duplicate specimens to the more eminent Societies and naturalists throughout Europe and America.

This immense labour has occupied Dr. Wallich for the last four years; and it is the chief selection from these various Herbaria, destined for the museum of the India House, which the Court of Directors have, with princely munificence, presented to the Linnæan Society.

The liberality of the East India Company has been duly appreciated throughout the wide circle of science. It has been acknowledged by letters and addresses from the different Societies and individuals honoured by their patronage; and this last act of their bounty will endear them still more to the promoters of Botany, by placing the treasures they possessed along with those of Linnæus and Smith.

The Linnæan Society purchased, two years ago, at an expense of 3000*l.*, the collections of Linnæus and of the late excellent Sir J. E. Smith; and since that the Herbarium of the Society has been further enriched by the treasures of the East, it forms collectively one of the most interesting and important in Europe.

The East India Company have set an example of a wise and liberal policy, which will be followed throughout the world, not only by Societies, but by those enterprising individuals who have, to their own honour, made large collections of the objects of natural history; and it is a source of national congratulation that at this moment the naturalists of Europe feel indebted to this country for the most extensive contribution that was ever made to their botanical collections. We owe this general feeling of respect towards us to the enlightened conduct of the Court of Directors, who have done more to diffuse a knowledge of Botany than was ever done by any Government or association of persons on the globe.

A deputation from the Council of the Linnæan Society, headed by the President Lord Stanley, waited on the Chairman of the Court of Directors, on the 26th instant, with an address expressive of the high sense the Society entertains of the honour conferred upon it by the liberality of the East India Company.

FRIDAY-EVENING PROCEEDINGS AT THE ROYAL INSTITUTION OF GREAT BRITAIN.

April 13.—Dr. Marshall Hall on the Laws which govern the mutual relation of Respiration and Irritability.

The object of this lecture was briefly to present the results of an

experimental investigation into the ratio which obtains between the quantity of the respiration and the degree of the irritability in the different series and forms of being in the animal kingdom.

The quantity of the respiration is expressed by the quantity of oxygen removed in a given space of time.

The degree of the irritability is denoted by the degree and duration of the contraction of the heart and of other muscular parts, on the application of a given degree of stimulus.

These two properties are always found to be in an inverse ratio to each other. In animals of a high respiration, as the birds and the mammalia, the degree of irritability is low; in animals of a low respiration, as the tortoises, the serpents, the batrachia, the degree of irritability is extreme.

It is absolutely necessary to distinguish *activity* from irritability. The former seems to depend upon the action of a highly arterial blood upon the nervous system, and is great in birds and the mammalia; the latter is inherent in the muscular fibre itself, and seems to result from its peculiar organization and condition at the time.

The law of inverse respiration and irritability applies not only to the different individuals of the animal series, but to the different forms of the same animal. The egg, the tadpole, the larva, have respectively a lower respiration and a higher irritability than the same beings in their subsequent higher states of existence.

The same law still obtains in animals which undergo a change of condition from the operation of some natural causes, as in the diurnation and hibernation of the bat; in torpor from cold in animals which do, or do not, hibernate; in the case of the privation of food, &c.

But the most extraordinary exemplification of this law is in the double heart of birds and of the mammalia itself. The left side of this organ, which receives a highly arterialized or respired blood, possesses a low degree of irritability; the right side, which receives blood of an unrespired character, possesses a high irritability: the latter continues to beat after the former has ceased its contractions.

Various deductions were drawn from these observations. It was shown, that with the high respiration and low irritability coincide, 1. great necessity for air and food; 2. a high animal temperature; 3. great activity; 4. little tenacity of life; and, 5. a greater power of bearing augmented than diminished stimulus in general:—And that with a high irritability and a low respiration co-exist, 1. the power of sustaining the privation of air, of food, &c.; 2. a low animal temperature; 3. little activity; 4. great tenacity of life; and, 5. little power of bearing the action of augmented stimulus. The former class are more injured by exposure to cold, the latter by heat.

The lecture was concluded by a hasty reference to the interesting facts detailed by Legallois and by Mr. Edwards, which, although given in an isolated form by those authors, admit of being readily explained and arranged by a reference to the law,—that the quantity of the respiration is inversely as the degree of the irritability, and to the corollaries which flow from it.

May 4.—Mr. Cottam on the application of cast-iron to bearing purposes, especially in the form of beams, girders, brackets, &c. &c. —After explaining that the elastic force of a bar was the utmost weight which it could bear, so that upon its removal the bar should return to its original form, and showing by experiment that this was very much less than the breaking force, he stated that it was of the utmost consequence to attend to the limit of elastic power. If the material be strained beyond that point, and the straining force be suffered to remain, or frequently repeated, the deflection continues to increase, and fracture ultimately takes place; but if the load be restrained within the limit of elastic power, it may be suffered to remain for any length of time with perfect safety, and without increasing the deflection in the smallest degree.

In the various experimental illustrations it appeared that a bar supported at both ends, when loaded with 189 pounds in the centre, or 236 pounds distributed at equal distances over its length, was equally deflected. When the length of the bar was reduced one half, the weight supported at the centre was doubled. A bar supported 378 pounds without receiving any set, *i. e.* it remained capable of returning to its original form when the weight was removed. This was the limit of its elastic power: it broke with 556 pounds.

From the various experiments the following practical rule was drawn. Multiply 850 times the breadth in inches by the square of the depth in inches, and divide the product by the length of bearing in feet; it will give the weight to be supported in pounds at the middle of its length, or twice that weight distributed uniformly over its surface.

May 11.—Mr. Cowper on recent improvements in the loom for weaving silk.—Mr. Cowper's object was principally to explain the construction of the Jacquand loom, and the nature of the innumerable changes therein produced, and which are requisite for any rich figured pattern. He had drawings and models of many preceding looms, and illustrated his details by numerous contrivances; but we are unable, without figures, to convey any notion of the beautiful principles which in this loom are put into practice.

Afterwards Professor Ritchie showed the inflammation of oxygen and hydrogen gases by the electric spark obtained by magneto-electric induction; and Mr. Faraday showed the spark itself to all present by means of the arrangement already described, Phil. Mag. and Annals, N. S. vol. xi. p. 405.

May 18.—Mr. Faraday on the crispations of fluids lying on vibrating surfaces.—This was a development and demonstration of the principles by which Mr. Faraday explains certain curious modifications of the forms of fluids supported on vibrating plates. The investigation forms part of a paper published in the Philosophical Transactions for last year, but not read before the Royal Society for want of time. We refer to the paper for details, and for the extensions of the principles which the author puts forth.

May 25.—Mr. Brockedon on the Pering anchor. The various improvements made in the form of the anchor by Mr. Pering have

been gradually introduced throughout our navy, and were fully described and illustrated by Mr. Brockedon, who made great use of drawings and models for the purpose. They consist essentially in using rolled iron in the form of plates, so placed that the strain shall be in the direction of their depth; and also in giving greater depth to the anchor as a whole, in the direction of the line of resistance. By these means anchors containing a certain weight of metal have, when opposed to much heavier anchors of the usual form, torn them to pieces. These improvements have all been patented, and also described elsewhere, so that it will be unnecessary to report more fully on them here.

June 1.—Mr. Faraday entered into an account of the principle of that most perfect of locks invented by the late Mr. Bramah, and of the apparatus belonging to Mr. Mordan, by which they are constructed. The apparatus (from Mr. Mordan's works) were in the room, their principles explained, and their efficacy and perfection illustrated by the performance of the various processes to which each was devoted.

June 8.—Mr. Edwards gave a full detail of the recent important improvements in lithotrity by Baron Heurteloup. He prefaced it by an account of the structure of the parts concerned, and of what others had done in this branch of medical science; after which he reviewed the instruments successively invented by Baron Heurteloup; and especially dwelt on the last, an instrument by which the calculus is seized, and then crushed and broken by blows of a hammer from the outside, without any distress to the patient. Baron Heurteloup himself, after the lecture, illustrated the mode of operation, upon real calculi, fully demonstrating the power and rapidity of operation of the instrument.

This was the concluding evening of the meetings for the present season.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

March 5, 1832.—The President (Prof. Sedgwick) in the chair. Among the presents were; a collection of British Insects from A. Badger, Esq. Trinity College, and a specimen of the Northern Diver, from Dr. Butler.

A paper was read "On a new analyser, and its use in experiments of polarization," by Prof. Airy.—The author mentioned this as an instance in which results deduced from Fresnel's theory had been confirmed by observation, and which therefore served to establish the correctness of that theory. The experiments were suggested by a consideration of the theoretical use of the analysing plate in the ordinary polarizing apparatus. The light which falls upon a crystalline plate has upon emerging from it the same intensity (neglecting the loss of light at the surfaces, &c.) as before it entered; and consequently no coloured rings can be seen. By the use of an analysing plate, coloured rings are seen in experiment; and the theoretical explanation of this is, that the analyser has resolved the light that emerges from the crystal into two streams of

light polarized in planes at right angles to each other, of which it has suppressed one, and transmitted the other to the eye. The calculation founded on this, represents correctly the phænomena. But it is plain that, allowing the truth of this general explanation, different kinds of analysis may be conceived:—of these, that which comes next in simplicity to the ordinary kind is, the resolution of the emerging light into two streams; one, circularly-polarized and right-handed; the other, also circularly-polarized, but left-handed; and the suppression of one of these streams. The effect of this analysis is not immediately obvious when the light incident on the crystal is plane-polarized; but when it is circularly-polarized, one remarkable result presents itself. As the only incident light has no relation to sides, and the only light allowed to emerge has no relation to sides, the coloured rings can have nothing which bears any trace of sides except in the crystal itself. Consequently there can be no black cross or black curve as with the usual apparatus (for the positions of these are determined by the planes of polarization), and therefore Iceland spar will produce circular rings; and nitre, &c. will give uninterrupted lemniscates. And as a general rule, the amount of light of any ray which comes to the eye will depend upon nothing but the difference of paths of the corresponding ordinary and extraordinary ray in the crystal. An analysis of this kind might throw considerable light on the mechanical state of unannealed glass, &c. The author then showed that such an analyser would be produced by combining the ordinary analysing plate with Fresnel's rhomb, or with a plate of mica of the proper thickness. The experiments (which were exhibited to the members of the Society present) corresponded completely with the anticipation. Allusion was also made to a more general kind of resolution; namely, into two streams of elliptically-polarized light; but this subject was not pursued by the author into the same details as the other.

A memoir by Mr. Murphy, of Caius College, was read. The author's object in this memoir is to furnish a general method, by which an unknown function, entering under the sign of definite integration, may be found from the known integral. Adopting the limits 0 and 1, he shows that in the equation $\int f(t) \, . t^x = \varphi(x)$, if we assign to x a series of values, from the greatest root of the equation $\varphi(x) = \infty^y$, to $x = \infty^y$; $\varphi(x)$ converges to zero as its limit, when $f(t)$ is any of the functions commonly received t^{-x} in analysis; and proves, that if we multiply, in this case, $\varphi(x)$ by and divide by t the coefficient of $\frac{1}{x}$ on the product, we shall obtain $f(t)$. To remove the difficulty presented by discontinuous functions, he considers the character of *least*, which distinguishes the root of an equation solved by the method which the author communicated in a former paper; and he finds in this circumstance the means of representing *explicitly* such functions under continuous forms. In applying the above principles to the distribution of electricity on the surfaces of bodies, the author has been led to new and remarkable results, on the nature of the accumulation, neces-

sary to produce any observed phænomenon. Thus when the external action of a spherical electrized body, in any direction, varies according to an exact inverse power of the distance, higher than the second, the opposite electricities will be arranged in pairs, in exactly equal quantities, between the neutral lines and the lines of greatest accumulation; and the influence of an external point will be as a constant quantity — the inverse cube of the distance.

After the meeting, Prof. Airy exhibited an apparatus illustrative of some of the phænomena referred to in his paper; and Professor Henslow gave an account, illustrated by drawings and specimens, of observations made on the age of trees.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

We have been favoured by a correspondent, who is a member of the Association, with the following notice of the meeting held at Oxford, from the 18th to the 23rd of June.

"We believe we speak the sentiments of a great majority of the individuals lately assembled at Oxford, when we say that this Meeting has been most satisfactory in every way,—that Oxford has performed her part nobly, and that the distinguished men who have been entertained within her ancient walls will long remember, with feelings of proud exultation, the week spent there. If the meeting at York was productive of so many valuable results, what may not be anticipated from an assembly of a much more numerous and splendid kind? If the head of Science was drooping, the Meeting at Oxford cannot but revive it. Science has here been honoured in a new way,—in a marked and preeminent manner; and every individual assembled at Oxford must have been gratified by the distinctions conferred on Brewster, Dalton, Faraday, and Brown.

"If the scientific men, drawn from the remotest corners of the kingdom, were gratified with the splendid hospitality both publicly and privately displayed,—in an equal degree, we have authority for saying, were the Vice-Chancellor, and the members of the University generally, pleased with the compliment paid to the University by the meeting of the Association there. Oxford has shown that she is favourable to knowledge;—that she is willing to bestow her honours on its most successful cultivators;—that she is bounded by no narrow and contracted notions in all that regards rank and condition in life, or the diversities of human belief, when the enlargement of the great empire of science is concerned;—that she has in the present instance set a magnificent example to the whole empire;—that by her splendid and refined liberality she calls upon the other Universities, upon the great commercial cities, upon London itself, to tread in her brilliant steps; that she has welcomed within her walls a body of enlightened men far more numerous than were ever assembled before for the purposes of science in these dominions; that these, stimulated by the feelings which an Association so novel cannot fail to produce, are returned or returning to their habitations with hopes revived and energies re-awakened,—no longer solitary and detached, but linked

and united in close communion with the greatest minds. The man of obscurity, but of great merit, in the provinces, becomes thus a new being, with feelings of intense power awakened into living activity, of which activity he was before unconscious;—he feels for the first time that HIS science,—the science which he has cherished and preserved in a narrow circle, is of practical value; that it has obtained the approbation of men to whom he has been always accustomed to look up with veneration and respect;—he forms a resolution to embark in new inquiries; and the individual who before pined away his hours in listless obscurity is suddenly summoned into a new state of being.

"The time of publication will not admit of our giving more than this very brief notice of the Meeting of the British Association this month; but, brief as it is, we cannot conclude it without saying that science has been highly honoured and benefited by it; that we look forward with feelings of intense pleasure to the next meeting at Cambridge; —but that wherever it may in future be held, the kind and splendid attentions of the venerable the Vice-Chancellor, of Dr. Buckland, and the members of the University generally, can never be effaced from the recollections of those who were assembled there.

"It ought not to be omitted, that a Meeting which began with the proudest sanction of Philosophy was dignified at its conclusion by the holy influences of Religion. The sermon of the Rev. Professor Mills, preached in the University Church on the Sunday immediately succeeding the Meeting, on Philosophic Humility, was distinguished at once by its lofty and commanding eloquence, and the impressive effect it produced on the great body of learned men assembled there. True religion and sound philosophy must ever go hand in hand; and it was gratifying to see that the enlightened views of the Divine were received with thankfulness and pleasure by the Philosopher. The sermon of the Rev. Professor was immediately requested to be printed."

G. H.

Monday, June 18th.—Many distinguished cultivators and admirers of science having assembled, in the course of this day meetings of the Committee and of the General Association were held, for the purpose of admitting new members, of which the numbers, both of strangers and of residents, were very considerable. Various arrangements were also made for the transaction of the business of the Association; and it was agreed that general meetings should be held each day at one o'clock, and that in the mornings and evenings the members should meet in four sections, each for the consideration of a certain division of science. The following were the arrangements thus made:

SUB-COMMITTEES.

I. *Sub-Committee of Mathematical and Physico-Mathematical Sciences* (*Astronomy, Mechanics, Hydrostatics, Hydraulics, Light, Heat, Sound, and Meteorology*).

Meetings for Committee business in the Clarendon Buildings, Room A, from Ten to Eleven.

Meetings for reading papers, &c. Room A, from Eleven to Twelve.

II. *Sub-Committee of Chemistry, Electricity, Galvanism, Magnetism, Mineralogy, and Chemical Arts and Manufactures.*

Meetings for Committee Business, Room B, from Ten to Eleven.
Meetings for reading papers, &c. Room B, from Twelve to One.

III. *Sub-Committee of Geology and Geography.*

Meetings for Committee business, Room C, from Ten to Eleven.
Meetings for reading papers, &c. Room C, from Eleven to Twelve.

IV. *Sub-Committee of Natural History, (Botany, Zoology, Physiology, Anatomy, Medicine, &c.)*

Meetings for Committee business, Room D, from Ten to Eleven.
Meetings for reading papers, &c. Room C, from Eleven to Twelve.

N.B. All members of the Association may attend the meetings at which papers are read.

The Committee of each Section will choose its own Chairman and Secretary on assembling, and substitutes for them when necessary. If it be found convenient, any Section may unite with another, or may subdivide itself into Subsections. The Chairman of the Committee is to preside at the Sectional Meetings, and to take charge of the business of the Committee in the intervals of the meetings, as well as during the meeting. The Chairman of each Section will be furnished by the Committee of Papers with a list of the communications to be laid before the Section, so far as they have been announced to the officers of the Association.

The communications which persons are invited to make to the sections, are "written or verbal announcements of recent discoveries, researches, results of researches, experimental decisions of disputed points, suggestions of important points to be examined, information of the progress of science in foreign countries, and oral remarks on such communications." The Secretary of each section or sub-section on being directed by the Chairman, is to make a memorandum of the subject of each communication made to the Section. These memoranda of the Secretaries of the Sections shall be communicated to the Secretaries of the Association, for the purpose of being published with the Report of the Meeting. The committees of each section are requested to suggest any subjects on which reports on the recent progress and present state of particular sciences from persons of known scientific eminence are desirable, or any specific inquiries, or observations in different places by societies or individuals, to suggest also the names of persons to be applied to for these purposes, and to communicate their suggestions to the General Committee on Saturday.

The Committees will communicate together respecting points on which they can be auxiliary to each other.

PROVISIONAL SUB-COMMITTEES.

I. *Mathematics and General Physics.*—Professor Airy, Professor Babbage, Sir D. Brewster, Sir T. Brisbane, Mr. Brunel, Rev. H. Coddington, Mr. Creswell, Mr. J. D. Forbes, Mr. Davies Gilbert, Professor Hamilton, Mr. Harvey, Professor Jarrett, Mr. Murphy,

Dr. Pearson, Professor Powell, Mr. Potter, Professor Rigaud, Mr. Rothman, Captain Smyth, Rev. R. Willis, Rev. W. Walker, Rev. W. Whewell.

II. *Chemistry, Mineralogy, &c.*—Mr. Dalton, Dr. Daubeny, Mr. Children, Professor Cumming, Mr. Faraday, Mr. Johnston, Dr. Prout, Dr. Turner, Rev. W. V. Harcourt, Mr. Harris, Professor Ritchie, Mr. Scoresby, Dr. Gregory, Mr. König, Mr. Brooke, Professor Miller, Marquis of Northampton, Mr. Guillemard.

III. *Geology and Geography.*—Rev. W. Buckland, D.D., Rev. W. Conybeare, Rev. A. Sedgwick, R. I. Murchison, Esq., G. B. Greenough, Esq., W. H. Fitton, M.D., Rev. W. V. Harcourt, The Marquis of Northampton, Major-General Straton, Viscount Cole, Sir Philip Egerton, Bart., William Smith, Esq., Dr. Edward Turner, Henry Witham, Esq., Thomas England, Esq., Sir C. Lemon, Bart, W. Hutton, Esq., W. Clift, Esq., John Taylor, Esq., Rev. J. Yates, G. Mantell, Esq., Sir T. D. Acland, Bart., J. Carne, Esq.

IV. *Natural History.*—Mr. R. Brown, Dr. Daubeny, Professor Henslow, Dr. Williams, Mr. Richard Taylor, Mr. Jenyns, Mr. Garnons, Mr. P. Duncan, Mr. Yarrell, Mr. Vigors, Mr. Sabine, Dr. Prichard, Mr. Clift, Dr. Kidd, Dr. Knox, Mr. Burchell.

The authorities of the University granted to the Association the use of the Sheldonian Theatre for the General Meetings, and of the rooms in the Clarendon Buildings for the Sectional Committees.

June 19. The Sectional Committees met at Ten o'clock, and appointed the following gentlemen to be their Presidents and Secretaries respectively.

Physics, &c.—*President,* Mr. Davies Gilbert; *Secretary,* Rev, H. Coddington.

Chemistry, &c.—*President,* Mr. John Dalton; *Secretary,* Mr. Johnston.

Geology and Geography.—*President,* Mr. Murchison; *Secretary,* Mr. J. Taylor.

Natural History.—*President,* Mr. P. Duncan; *Secretary,* Professor Henslow.

At one o'clock the Association met in the Theatre: Viscount Milton (the President of the Association at the former Meeting, and President of the Yorkshire Philosophical Society,) took the chair, and opened the business of the Meeting, proposing that Dr. Buckland, (the President Elect for the present Meeting,) should take the chair as President.

Professor Airy then read his Report on the State and Progress of Physical and Practical Astronomy.

The Rev. W. Whewell next stated, in the absence of the author, the substance of Mr. Lubbock's Report on the Present State of our Knowledge respecting the Tides, which was illustrated by the exhibition of a map of the world, on which were drawn the *Cotidal Lines,* or lines which pass through all the points at which it is supposed to be high water at the same moment.

At five a dinner was given to the Association, in the Hall of New College, by those members of it who are resident in Oxford:—in

the evening the Association adjourned to the Clarendon Rooms, where Sectional Meetings were held. Mr. Hemming made before the Sectional Committee on Chemistry, some experiments, exhibiting the action and use of his safety-tube for the oxy-hydrogen blow-pipe, noticed among our Miscellaneous Articles in the next page.

June 20.—This morning begun with a public breakfast, given by the Vice-Chancellor, in Exeter College hall, and gardens, to all the Members of the Association. At the General Meeting immediately following, the Chairman of each Sectional Committee read the Report of the proceedings of the preceding day in his respective department. At the conclusion of the Report of the Geological Section, the President requested permission of the assembly to allow the Wollaston Gold Medal,—awarded last year by the Council of the Geological Society to Mr. W. Smith, but which had not then been executed,—to be presented to that gentleman in the presence of the members of the Association. This was accordingly done by Mr. Murchison, who thus addressed Mr. Smith: "To you, William Smith, who have, by the universal voice of Geologists, been pronounced the Father of English Geology, I have the sincerest pleasure, in the name and on the behalf of the Geological Society, in presenting this Medal."

Mr. Smith briefly returned thanks for the honour thus conferred upon him.

Professor Cumming then read his Report on the progress of Thermo-electricity.

Mr. Forbes read his Report on the present condition of our knowledge in Meteorology.

The Rev. R. Willis gave a verbal account of the present state of the Philosophy of Sound, illustrated by diagrams and experiments.

In the evening two lectures were given in the Music room; one, by Dr. Ritchie, on Magneto-Electricity; the other, by Dr. Turner, on certain points of Chemical Science.

June 21.—This morning the Vice-Chancellor of the University and the Heads of Colleges entered the Theatre in procession; and the Vice-Chancellor opened the business of the Convocation in the usual academical form. Davies Gilbert, Esq. M.P., V.P.R.S., &c. took his seat for the first time as Doctor of Civil Law, a distinction which has recently been conferred upon him by diploma. The Professor of Civil Law (Dr. Phillimore) then presented to the Convocation the four men of science whom it was proposed to admit on this occasion to the degree of Doctor of Civil Law,—viz. Sir David Brewster; R. Brown, Esq., V.P.L.S., &c.; John Dalton, Esq., F.R.S.; and Michael Faraday, Esq., F.R.S., &c.—in an elegant and discriminative Latin speech, stating what the University considered their respective claims to distinction.

The following members of the Universities of Cambridge and Dublin were then admitted *ad eundem* of the University of Oxford:—John Read Corrie, M.D., of Gonville and Caius College; Thomas Smith Turnbull, M.A., Fellow of Gonville and Caius College; John Blackburn, M.A., St. John's College; Rev. Robert Willis, Fellow of

Gonville and Caius College; Edmund Storr Halswell, M.A., St. John's College; William Garnons, M.A., Fellow of Sidney College; Henry Edward Fawcett, M.A., Trinity College; W. Miller, M.A., St. John's College, Professor of Mineralogy; James Bowstead, M.A., Fellow of Corpus Christi College; Walker Gray, M.A., St. John's College; James Cumming, M.A., Trinity College, Professor of Chemistry; James Dunn, M.A., Trinity College, Dublin.

Later in the same morning a number of members of the Association assembled near Magdalen Bridge to accompany Professor Buckland on an excursion of some hours, in the course of which he explained the geology of the neighbourhood of Oxford.

In the course of the session the following Members were elected Council and Officers for the ensuing year, who will hold their meetings occasionally in London:—

President—Dr. Buckland. *Vice-Presidents*—Sir D. Brewster, Rev. W. Whewell. *President Elect*—Rev. Professor Sedgwick. *Vice-Presidents Elect*—Dr. Dalton, Professor Airy. *General Secretary*—Rev. W. Vernon Harcourt. *Assistant Secretary*—John Phillips, Esq. *Treasurer*—John Taylor, Esq. *Trustees*—Professor Babbage, Roderick Impey Murchison, Esq., John Taylor, Esq. *Council*—Dr. R. Brown, M. I. Brunel, Esq., William Clift, Esq., Rev. J. Corrie, J. D. Forbes, Esq., Davies Gilbert, D.C.L., J. H. Green, Esq., G. B. Greenough, Esq., Sir John Herschel, Professor Hamilton, Professor Hooker, J. F. W. Johnston, Esq., Dr. Lloyd (Dublin), Dr. Luby (ditto), Rev. J. Peacock, Dr. Pritchard, J. Robison, Esq., Rev. W. Scoresby, Rev. J. J. Taylor, Dr. Traill, N. A. Vigors, Esq. *Secretaries to the Council*—Dr. E. Turner, Rev. James Yates. *Secretaries at Cambridge*—Professor Henslow, Rev. W. Whewell. *Secretaries at Oxford*—Dr. Daubeny, Rev. B. Powell.

We hope to give in our next Number a complete list of the Reports of the progress of various branches of science which were read during the session, and which, we understand, will be published in a volume, together with the Report of Proceedings.

XVIII. *Intelligence and Miscellaneous Articles.*

SAFETY-TUBE FOR THE COMBUSTION OF THE MIXED GASES OXYGEN AND HYDROGEN, INVENTED BY MR. HEMMING.

A CYLINDER about six inches long and three-quarters of an inch wide, filled with very fine brass wires, in lengths equal to the tube. A pointed rod of metal, one-eighth of an inch thick, is then forcibly inserted through the centre of the bundle of wires in the tube, by which they are wedged more closely together. The interstices between the wires, which are exceedingly small, are then in effect a series of metallic tubes of very minute diameter: the cooling and conducting power of these is far greater than could be produced if a cylinder of equal length were filled with discs of wire gauze, as the apertures are much smaller than those in the finest gauze, and there

is unbroken continuity. All attempts to produce explosion of the gases in this tube, or to compel the flame to return through it, have been ineffectual. Before the Society of Arts, Mr. Hemming exploded the gases repeatedly in the improved safety chamber, now employed in Gurney's blow-pipe, by permitting small portions of water from the well to enter with them, but he could not explode them in his improved tube under precisely the same circumstances, although they were ignited at the aperture (nearly three-quarters of an inch in diameter,) after the jet piece was removed.

Mr. Hemming kept the gases ignited at this large aperture until the extremity of the tube was in a state of active combustion, which was evident by the dense green flame produced; and although the cooling influence was then greatly diminished, no explosion occurred.

The simplicity of its construction will render the manufacture of the article easy and œconomical; and its perfect safety will enable the chemical operator to dispense with a very expensive and delicate article of apparatus, in the use of which there is always danger and uncertainty.

CONVERSION OF HYDROCYANIC ACID AND CYANURETS INTO AMMONIA AND FORMIC ACID.

M. Pelouze concludes, from numerous experiments,—

1st, That hydrocyanic acid is converted into ammonia and formic acid by the action of the sulphuric and muriatic acids, and probably by a great number of other acids.

2ndly, That cyanuret of potassium, when a concentrated solution of it is heated, is changed into ammonia and formiate of potash.

3rdly, That the same salt at a high temperature, when influenced by an excess of potash, yields hydrogen, ammonia, and a residue of carbonate of potash.

4thly, That one proportion of cyanuret of mercury acting upon one proportion of muriatic acid, gives one proportion of hydrocyanic acid, and one of perchloride of mercury.

5thly, That cyanuret of mercury with an excess of muriatic acid produces double chloride of ammonia and mercury, formic acid, and a very little hydrocyanic acid.

6thly. That formiate of ammonia, when heated to about 360° Fahrenheit, is converted into water and hydrocyanic acid.—*Ann. de Chim. et de Phys.* vol. xlviii. p. 395.

ISOMERIC MODIFICATION OF TARTARIC ACID. BY M. BRACONNOT.

It is well known that the tartaric and racemic (paratartaric) acids were the first well-defined examples of isomerism. The judicious reflections of M. Dumas on this extraordinary phænomenon have recalled a fact belonging to it, and which I had occasion to observe, respecting tartaric acid.

Forty parts of this acid having been exposed for an instant to a considerable heat, they fused, swelled up, and left after cooling a dry yellowish matter, which was transparent like gum, and weighed 36·5 parts. This substance when softened by heat acquired great ductility, which allowed it to be drawn into threads as fine as hairs.

This change of form, which recalls the dimorphism of sulphur, shows either a new molecular arrangement, or another isomerical modification. In fact, the tartaric acid thus submitted to the action of heat, no longer possesses its original properties; it is uncrystallizable, and is merely a thick viscid mucilage, which attracts moisture from the air.

If this substance be dissolved in hot water, and carbonate of lime be gradually added to saturate it, it does not form, as with common tartaric acid, a sandy deposit of crystallized tartrate of lime, but the solution becomes gradually turbid as it cools, and deposits a mucilaginous transparent insipid mass, which forms threads between the fingers like turpentine. This calcareous salt when dried is unalterable in the air, and resembles gumarabic. When heated in water or weak acetic acid, it softens, resuming its viscid and adhesive properties, without being sensibly dissolved; an excess of acid, however, redissolves it, especially when hot, and by evaporating the solution to dryness, there remains a dry brittle acidulous substance, which is transparent like a varnish, is unalterable by the air, and which when immersed for some time in cold water seems to undergo a molecular motion, which reproduces tartaric acid in its original state, for then there separates a sandy deposit of common tartrate of lime.

Tartaric acid modified by heat, also dissolves magnesia and yields a bitter liquor, which leaves a varnished surface by evaporation. Crystallized tartaric acid acts quite differently with this earth, for it immediately precipitates a white powder, which is difficultly soluble in water; the same modified acid, saturated with soda, produces an uncrystallizable mucilaginous combination, which attracts moisture from the air.

With potash an analogous result is obtained; and if to the compound an acid be added in excess, a very greatly divided precipitate is formed, as difficultly soluble as tartar, but which has not its granular appearance. When redissolved in hot water, it gives by cooling white opake plates, in which rudiments of crystals are scarcely discernible; this acidulous salt when saturated with soda gives a salt analogous to Rochelle salt. Although tartaric acid, when exposed to heat, is not a very permanent isomeric substance, it evinces at least a remarkable tendency to this state. — *Ann. de Chim. et de Phys.* xlviii. 299.

FORMATION OF CARBONATE OF LIME UNDER THE INFLUENCE OF SUGAR. BY M. PELOUZE.

Mr. Daniell concluded from his experiments, that when lime is dissolved in an aqueous solution of sugar, the sugar is decom-

posed, and converted into mucilaginous matter, while the lime is precipitated in the state of carbonate, and crystallized in very acute rhomboids. M. Becquerel obtained the same crystals, under the influence of weak electrical currents; and as he operated without the contact of air, the decomposition of sugar was undoubtedly effected, under the circumstances in which it was placed.

M. Pelouze concludes from new experiments which he has made on this subject, that when the mixture is exposed to the air, the sugar is not decomposed, and that the crystals which are produced, result from the action of the carbonic acid of the atmosphere upon the lime; and the carbonate forming slowly in a fluid, is deposited with water of crystallization. It contains 5 atoms of water; at about 86° of Fahrenheit it loses its water of crystallization and becomes pasty; but what is very remarkable is, that the salt which is perfectly dehydrated at 86° when it is in water, loses only 2 atoms when it is heated in strong alcohol at a boiling heat. The new salt containing 3 atoms of water is efflorescent in the air, while that with 5 atoms undergoes no alteration.—*Journal de Pharmacie*, April 1832.

EXTEMPORANEOUS SOLUTION OF CHLORINE.

M. Tourtois gives the following quantities of ingredients for obtaining a solution of chlorine, which are to be added to an imperial quart of water and well shaken together in a stopped bottle; and he remarks that unless the deutoxide of lead be finely powdered, some of it will remain undecomposed:—

Sulphuric acid	910 grains
Common salt	280
Deutoxide of lead	840—*Ibid.*

As, however, 280 of common salt contain 112 of sodium, requiring nearly 38 of oxygen for conversion into soda, and as 116 of deutoxide of lead give out only 4 of oxygen by reduction to protoxide, it will appear by calculation that 1102 grains of red-lead should be used with 280 of salt, instead of only 840. The sulphuric acid must be equivalent to 150 of soda and 1064 of protoxide of lead, or about 700 grains, instead of 910.—R. P.

SEPARATION OF PEROXIDE OF IRON FROM PROTOXIDE OF MANGANESE. BY M. LIEBIG.

When carbonate of lime is boiled with a solution of peroxide of iron and protoxide of manganese, the former is precipitated, and the latter remains in solution; the separation is so complete that no trace of iron remains in solution, nor is any manganese precipitated.

Carbonate of magnesia may be employed for the same purpose. To determine the precision of this method, one part of protosulphate of manganese was mixed with forty parts of protosulphate of iron, and mixtures were made in inverse proportions; after having peroxidized the iron by nitric acid, the solutions were boiled with carbonate of magnesia.

In every case the oxide of iron was completely precipitated, and without a trace of oxide of manganese. The muriates and nitrates of these oxides were similarly treated, and the results were similar, both with the carbonate of lime and with that of magnesia.

SEPARATION OF PEROXIDE FROM PROTOXIDE OF IRON. BY M. LIEBIG.

The most distinguished analysts have been occupied with finding a method of separating the oxides of iron. The process proposed by Fuchs is extremely accurate; mixtures of proto- and per-salts of iron are boiled with carbonate of lime; the peroxide of iron is precipitated in the state of a subsalt, and so completely that the solution is not turned red by the sulphocyanate of potash. The only inconvenience of the process is, that the filtered solution, being perfectly neutral, becomes slightly turbid, owing to the conversion of a small portion of protoxide into peroxide. But this may be avoided by using carbonate of magnesia instead of carbonate of lime; the solution does not become turbid, and probably because magnesia forms a more stable double salt with the protoxide of iron.

In some applications this method of separation may be of importance. Calico-printers, as is well known, employ pyrolignite of lime to produce very different tints, and these depend upon the proportion of peroxide which it contains, and this is easily determined by the following process: Take two equal portions of pyrolignite of lime; one of them is peroxidized by means of a solution of chlorine, or by ebullition with nitric acid; then precipitate by ammonia, which gives the entire quantity of iron dissolved; the other portion is to be boiled with carbonate of magnesia, and then filtered; the protoxide of iron is afterwards converted into peroxide by the means above mentioned, and precipitated by ammonia, after having added a certain quantity of muriate of ammonia to prevent the precipitation of the magnesia. The weights of these two precipitates, after subtracting the second from the first, will give with sufficient accuracy the proportions of prot- and per-oxide.

SEPARATION OF PEROXIDE OF IRON FROM THE OXIDES OF COBALT AND NICKEL. BY THE SAME.

Neither carbonate of magnesia nor that of barytes can be employed for this separation, because the salts of these two bases are entirely decomposed, and the oxides precipitated by them. But carbonate of lime may be advantageously used.

PREPARATION OF METALLIC CHROME. BY THE SAME.

When dry ammoniacal gas is passed over the triple compound of chloride of chrome and ammonia, heated to redness in a glass tube, it is completely decomposed, and black pulverulent metallic chrome is obtained, which acquires a metallic lustre when burnished, burns when heated to redness, and gradually going out becomes a brown powder.

If chloride of chrome be saturated with ammoniacal gas, the combination sometimes takes place with ignition; the vessel is filled with a purple red light, which continues till the chrome is saturated.

Metallic chrome may be obtained in a still simpler manner, by reducing chloride of chromium with ammoniacal gas in the same way; the metal is not then black, but of a chocolate-brown colour.

The preparation of chloride of chromium is so well known, that it would appear superfluous to say anything more about it if it were not in itself interesting. When a neutral solution of oxide of chrome in muriatic acid is evaporated, there is obtained, as is well known, a green mass, which does not alter at the temperature of boiling water, and even at some degrees above it; but when the temperature is very considerably raised, it begins to swell, and losing water is converted into a spongy crystalline brilliant mass, of a peach-blossom colour, which may be taken for a sublimate; but it is not one, for this compound is not volatile. The conversion of a muriate into a chloride cannot be shown in a more convincing manner by any other combination.

When the chloride of chrome is calcined in contact with the air, it is converted into a green oxide; but the colour is so fine that it may be interesting for the manufacturers of porcelain to be more particularly acquainted with the fact.

If sulphuretted hydrogen gas be passed over chloride of chrome, a brilliant black crystalline sulphuret of chrome is obtained.

Metallic chrome prepared in the manner above described, does not alter at a red heat; nor does it by continued calcination become green, which, however, might be expected to happen if the metal contained any admixture of chloride. I have not further examined whether the oxide obtained differs in its composition from common green oxide. If chloride of chrome be fused in proper proportion with muriate of ammonia and carbonate of soda, metallic chrome is not obtained, according to Wöhler, but protoxide of chrome, in crystalline scales, and green transparent crystals of common salt, which have a fine green colour, and apparently combined with chloride of chrome.—*Ann. de Chim. et de Phys.* xlviii.

LUNAR OCCULTATIONS FOR JULY.

Occultations of Planets and fixed Stars by the Moon, in July 1832. Computed for Greenwich, by Thomas Henderson, *Esq.*; *and circulated by the Astronomical Society.*

1832.	Stars' Names.	Magnitude.	Ast. Soc. Cat. No.	Immersions.				Emersions.			
				Sidereal time.	Mean solar time.	Angle from North Pole.	Angle from Vertex.	Sidereal time.	Mean solar time.	Angle from North Pole.	Angle from Vertex.
				h m	h m	°	°			°	°
July 11	33 Sagittarii	6	2178	22 14	14 54	150	180	Under	horizon		

Meteorological Observations made by Mr. THOMPSON *at the Garden of the Horticultural Society at Chiswick, near London; by Mr.* GIDDY *at Penzance, and Mr.* VEALL *at Boston.*

Days of Month, 1832.	Barometer.					Thermometer.					Wind.			Rain.			Remarks.
	London.		Penzance.		Boston	London.		Penzance.		Bost.	Lond.	Penz.	Bost.	Lond.	Penz.	Bost.	
	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	8½ A.M.							
● May 1	29·460	29·356	29·178	29·172	29·01	60	44	59	44	48	S.	SW.	calm	0·51	...	...	*London.*—May 1. Heavy rain. 2. Rain: fine. 3. Rain: strong gale from S.W. 4. Rain: clear. 5. Rain. 6. Cloudy: rain. 7. Heavy peal of thunder early A.M: sultry: temperature remarkably high for the period of the season. 8. Fine. 9, 10. Cold and dry. 11. Bleak: cold rain at night. 12. Cold and dry. 13. Cold and stormy, with some hail. 14. Fine. 15, 16. Fine: rain at nights. 17. Heavy rain, with thunder. 18—23. Fine. 24, 25. Hazy and sultry. 26. Cloudy. 27, 28. Fine. 29—31. Rain. Till the middle of the month the nights were cold and unseasonable, and often frosty.
2	29·422	29·356	29·263	29·175	28·80	62	48	59	48	52	S.	SW.	calm	·09	0·570	0·32	
3	29·453	29·375	29·316	29·292	28·80	55	45	56	49	56	SW.	SW.	W.	·08	·700	·38	
4	30·071	29·708	29·916	29·566	29·12	53	33	56	47	51	S.	NW.	N.	·10	·300	·90	
5	30·131	30·040	29·922	29·916	29·52	58	53	58	47	52	NE.	SW.	W.	·09	·475	...	
6	30·064	30·046	29·922	29·916	29·34	68	51	58	51	58·5	S.	W.	calm	·06	...	·10	
☽ 7	29·913	29·817	29·919	29·872	29·25	79	45	60	50	60	S.	NE.	W.	...	...	...	
8	30·165	30·022	29·978	29·972	29·30	67	37	58	47	57	W.	NE.	W.	...	...	...	
9	30·378	30·302	30·172	30·028	29·74	51	36	55	45	46	W.	NE.	E.	...	...	...	
10	30·437	30·413	30·419	30·384	29·90	51	32	52	40	44	W.	NE.	N.	...	...	...	
11	30·371	30·154	30·381	30·352	29·82	57	39	55	42	48	NW.	NE.	NW.	·05	...	...	*Penzance.*—May 1. Fair: rain. 2. Fair: showers. 3. Rain: fair. 4. Fair. 5. Rain. 6. Misty. 7. Fair: misty: clear. 8—11. Clear. 12. Fair: clear. 13. Fair. 14. Fair: rain at evening. 15. Fair: clear. 16. Clear: fair. 17. Fair: rain. 18. Rain: fair. 19. Fair. 20. Showers: fair. 21, 22. Misty: rain: fair. 23. Clear: fair. 24. Fair: clear. 25—27. Clear. 28. Fair. 29. Clear. 30. Rain throughout. 31. Fair.
12	29·983	29·773	30·051	29·922	29·43	53	37	52	45	47·5	NW.	NE.	N.	...	...	...	
13	29·826	29·736	29·914	29·908	29·25	55	34	51	44	47	N.	N.	NW.	·03	...	·18	
○ 14	29·829	29·785	29·917	29·748	29·36	55	40	51	43	45	N.	NW.	NE.	...	·390	...	
15	29·861	29·809	29·828	29·734	29·35	57	35	52	44	48	NE.	NE.	E.	·23	...	...	
16	29·896	29·865	29·888	29·884	29·44	58	38	54	45	47	N.	NE.	calm	·07	...	...	
17	30·027	29·887	29·978	29·914	29·50	55	41	54	43	53	NE.	SE.	E.	·18	·180	...	
18	30·219	30·111	30·011	29·984	29·65	63	35	57	44	54	SE.	SE.	calm	...	·350	...	
19	30·253	30·234	30·031	30·022	29·71	71	39	58	44	58	NE.	SE.	calm	...	...	·23	
20	30·289	30·201	30·016	29·978	29·60	66	44	62	50	58·5	E.	NW.	calm	...	...	...	
21	30·288	30·226	30·166	30·075	29·64	73	48	60	52	58	NE.	NW.	calm	...	...	...	*Boston.* — May 1. Cloudy: rain P.M. 2. Cloudy: rain early A.M.: rain, with thunder and lightning P.M. 3. Cloudy: heavy rain P.M. 4. Fine. 5. Cloudy: rain P.M.: cloudy all day—could not observe the transit of Mercury. 6, 7. Cloudy. 8. Fine. 9—11. Cloudy. 12. Stormy: rain P.M. 13. Stormy. 14—16. Cloudy. 17. Fine. 18. Fine: rain P.M. 19, 20. Fine. 21. Fine: rain P.M. 22—27. Fine. 28. Cloudy: rain early A.M. 29. Cloudy: rain A.M. 30. Fine: rain P.M. 31. Cloudy.
☾ 22	30·246	30·221	30·216	30·172	29·54	69	44	61	50	61	W.	NW.	calm	...	·200	·04	
23	30·287	30·275	30·222	30·216	29·66	70	45	64	47	61·5	NW.	NW.	calm	...	...	...	
24	30·300	30·256	30·219	30·210	29·66	73	46	65	49	63	E.	NE.	calm	...	...	...	
25	30·213	30·100	30·206	30·180	29·50	73	48	62	48	63	SE.	NE.	calm	...	...	...	
26	30·004	29·988	30·016	29·957	29·42	68	43	68	48	62·5	NE.	NE.	calm	...	...	...	
27	30·000	29·948	29·910	29·854	29·48	69	41	67	48	61	E.	SW.	calm	...	...	...	
28	29·949	29·902	29·860	29·834	29·42	74	45	66	53	60	SE.	W.	calm	...	...	·10	
29	29·938	29·902	29·880	29·866	29·36	71	45	60	52	58	S.	N.	W.	·32	...	...	
● 30	29·913	29·730	29·818	29·513	29·42	70	49	60	47	60	SW.	SW.	NE.	·22	1·060	·10	
31	29·527	29·469	29·486	29·466	29·00	59	42	60	55	57	S.	W.	NW.	·13	...	·35	
	30·437	29·356	30·419	29·172	29·41	79	32	68	40	54·7				2·16	4·225	2·70	

THE

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XIX. *On the Effect of Compression and Dilatation upon the Retina.* By SIR DAVID BREWSTER, *K.H. LL.D. F.R.S. V.P.R.S. Ed.**

THE production of light by a gentle pressure upon the eyeball, or by a sudden stroke upon the eye, is a fact which has been long known, but which, so far as I know, has never been carefully examined. In the sixteenth Query, at the end of his Optics, Sir Isaac Newton describes the fact, and reasons upon it in the following manner:

"When a man in the dark presses either corner of his eye with his finger, and turns his eye away from his finger, he will see a circle of colours like those in the feather of a peacock's tail. If the eye and the finger remain quiet these colours vanish in a second, but if the finger be moved with a quavering motion they appear again. Do not these colours arise from such motions excited in the bottom of the eye by the pressure and motion of the finger, as at other times are excited there by light for causing vision? And do not the motions once excited continue about a second of time before they cease? And when a man by a stroke upon his eye sees a flash of light, are not the like motions excited in the retina by the stroke? And when a coal of fire moved nimbly in the circumference of a circle makes the whole circumference appear like a circle of fire, is it not because the motions excited in the bottom of the eye by the rays of light are of a lasting nature, and continue till the coal of fire in going round returns to its former place? And, considering the lastingness of

* Read before the British Association at Oxford, June 22, 1832.

the motions excited in the bottom of the eye by light, are they not of a vibrating nature?" . . . .

The circle of light referred to in this passage always appears opposite to the point of pressure, and its centre has the same visible direction as that of a ray of light incident on the centre of the compressed portion of the retina. The reason why the phænomenon is best seen by turning the eye away from the finger is, that the retina is thus brought under the point of pressure; for if the eye remains at rest, or is turned towards the finger, the luminous circle is either imperfectly seen or disappears altogether, because the finger then presses, either wholly or partly, upon a part of the eyeball beneath which the retina does not extend. Sir Isaac Newton is mistaken in saying, "that the colours vanish in a second when the eye and the finger remain quiet." They undoubtedly continue as long as the pressure is kept up; and in proof of this I may mention a case which I had occasion particularly to study, in which the patient constantly saw the luminous circle, in consequence of an excrescence on the inside of the eyelid, which produced a continued pressure on the eyeball.

Sir Isaac Newton has not named the colours which he saw in the luminous circle, any further than by saying that they are like those in the feather of a peacock's tail. Although I have made the experiment a thousand times, under all varieties of circumstances, I have never been able to observe any other circles but *black* and *white* ones, with the exception of a general *red* tinge which is seen when the eyelids are closed, and which is produced by the light which passes through them. . . . .

When a gentle pressure is first applied so as to compress slightly the fine pulpy substance of the retina, a circular spot of colourless light is produced, though the eye is in total darkness, and has not been exposed to light for many hours. If light is now admitted to the eye, the compressed part of the retina is more sensible to the light than any other part, and consequently appears more luminous. Hence it follows, *that a slight compression of the retina increases its sensibility to the light which falls upon it, and creates a sensation of light when the eye is in absolute darkness.* . . . .

If we now increase the pressure, the circular spot of light gradually becomes darker, and at last black, and is surrounded with a bright luminous ring of light. By augmenting the pressure still more, a luminous spot appears in the middle of the central dark one, and another luminous spot diametrically opposite, and beneath the point of pressure. Considering the eye as an elastic sphere filled with incompressible fluids, it is obvious that a ring of fluid will rise round the point depressed

by the finger, and that its pressure from within outwards will *dilate* the part of the retina under the finger which was formerly compressed, and will compress all that part of the retina in contact with the elevated ring. An increase of pressure will be resisted by the opposite part of the retina, and will thus produce a compression at both extremities of the axis of pressure, occasioning the diametrically opposite spot of light, and also the luminous spot in the middle of the circular black space. Hence we conclude, *that when the retina is dilated under exposure to light it becomes absolutely blind, or insensible to all luminous impressions.*

These properties of the retina often exhibit themselves involuntarily when the body is in a state of perfect health. When we move the eyeball by the action of its own muscles, the retina is affected beneath the place where the muscles pull the eyeball; and there may be seen opposite each eye and towards the nose two semicircles or crescents of light; and other two extremely faint towards the temples. At particular times when the retina is more sensible than at others, these crescents become complete circles or rings of light. From the same cause, in the act of sneezing, gleams of light are emitted from each eye, during both the inhalation of the air and its subsequent expulsion; and in blowing air violently through the nostrils two patches of light appear above the axis of the eye, and in front of it; while other two luminous spots unite into one, and appear about the point of the nose, when the eyes are turned in that direction.

The phænomena which have been described are those produced by the parts of the retina which are most affected by any given pressure; but it is obvious that this pressure is propagated over the whole retina;—and it is a curious fact, that though this pressure is too weak to produce a luminous impression, it has yet power to modify other impressions previously made upon the membrane. If, from looking at the sun, the eye sees a *pinkish-brown* spectrum, a pressure upon another part of the retina will change it to a *green* spectrum; and when the pressure is removed it will again become *brown*. If the pressure is such as to diminish the sensibility of the retina, it will either diminish or entirely remove a weak spectral impression.

When the eye is pressed in front by putting the finger on the eyelid above the cornea, no luminous spectrum is seen; and I have not ventured to make the pressure sufficiently strong to make an impression on the back of the eye. I know a case, however, in which this effect was produced accidentally. A person, in a state of intense grief, had been sitting for some time with his hand pressed against his eye;—the moment the

hand was removed and the eye opened, a black spot, the size of a sixpence, was seen in the axis of vision.

It is from pressures on the retina that those floating masses of light are produced, which appear in particular states of indisposition. In affections of the stomach the pressure of the blood-vessels upon the retina is shown in the dark by a faint blue light, floating before the eye and passing off at one side. As the pressure increases, the blue light becomes *green*, then *yellow*, and sometimes even *red*, all these colours being occasionally seen at the edge of the luminous mass.

The preceding observations on the influence of dilatation in making the retina insensible to light, render it extremely probable that the disease in that membrane, called *Amaurosis*, may sometimes arise from a general distention of the eyeball, arising from a superabundance of the fluids which it incloses. If this be the case, the removal of the pressure might be effected by puncturing the eyeball (where it can be done with safety), and letting out a portion of the aqueous humour. How far such an operation would be effectual when the disease has been of long standing can be determined only by experiment.

XX. *A Sketch of the Geology of Six Miles of the South-east Line of the Coast of Newcastle in Australia;—with a Notice of three Burning Cliffs on that Coast. By the Rev.* Charles Pleydell Neall Wilton, *M.A. of St. John's College Cambridge, Fellow of the Cambridge Philosophical Society, and Chaplain of Newcastle*.*

THE whole line of the cliffs from the point (a) to (b) in the accompanying figure, affords a fine field for the operations of the geologist; and though in some parts the abruptness of the rocks, which project at their base into the sea, and in others the stupendous masses, which have from time to time (by the violent winds of the south-east gales, which prevail on this coast), been detached from the overhanging precipices, and been hurled to the bottom, impede the naturalist in his progress,—yet is his toil amply rewarded by the interesting results of his several investigations.

The height of the cliffs in general varies from about 100 to 300 feet, and their surface towards the sea presents in some places *three* and in others *two* parallel horizontal beds of coal, of the independent formation (with sometimes an occasional dip), having alternating layers of shale, *breccia*, more or less compact chert, sandstone, millstone-grit, clay-stone, slaty clay, clay-ironstone, and thin laminæ of ironstone divided into

* Communicated by the Author.

square and variously shaped sections. Large stems of arundinaceous plants in ironstone appear in great abundance between the horizontal beds of coal and the other strata, and impressions of fern-leaves, and of arundinaceous plants, of a size inferior to those in the ironstone, are found in different parts of the cliffs in six several formations; viz. in shale, ironstone, grit-stone, fuller's earth, grayish clay-stone, and in a fine red indurated clay-stone of a conchoidal fracture.

The Appearance of the Cliffs from the Telegraph Hill near the Entrance of the Harbour of Newcastle, for about Six Miles on the South-east Coast.

a The Lighthouse on the Telegraph Hill, near the entrance to the Harbour of Newcastle.
b The Cliff as it appeared when burning, on its discovery in the early part of 1830.
c The locality of the Cliff, which has been recently (August) discovered to have been on fire at no distant period.
d The locality of a third extinct fire, discovered September 15, 1831.

The summit of the hill at the point (*a*) on which the telegraph stands, near the entrance of the harbour of Newcastle, at about a foot beneath the surface on the face of the cliff, is covered with the trunks of petrified trees lying in a horizontal position, of a beautifully white texture, and finely grained, traversed with thin veins of white and bluish chalcedony, which is in some cases mammillated, the *bark* of these trunks being most correctly preserved. In some places on this coast the *white external* coat of the bark is retained, while the body of the wood itself is converted into ironstone.

At no great distance from the point (*c*) towards (*d*) beneath a stratum of breccia, varying from 8 to 30 feet in thickness, a bed of *brown* coal reposes, which passes into black, having immediately above it an accumulation of arundinaceous plants mixed with petrified wood; and above the latter formation a layer, 6 inches in thickness, of *fuller's earth*, of a greenish white, which lies directly under a bed of the same mineral, of a grayish and cloudy colour, containing numerous impressions of fern-leaves, and having a considerable thickness. The layers of fuller's earth are upwards of 100 feet in length, and are situated near the summit of the cliffs about 300 feet above the

sea. The other side of the cliffs inland, is found to contain large blocks of petrified wood, of a grayish-white colour passing into brown. About two miles from this spot towards the point (*b*), at the base of the cliffs may be seen large portions of the trunks of petrified trees, which have been detached from the heights above; and which upon being broken present a deep black appearance, capable of a very fine polish: the bark of the wood, which is exceedingly compact, and not easily fractured by the hammer, being of a grayish-white. Petrified wood in ironstone and hornstone, and traversed by fine veins of chalcedony, in some specimens beautifully crystallized, is found from two to three miles inland from the coast, lying scattered about in large blocks over the ridges, covered with thick and unfelled *bush*, and on the sides of the gullies and deep ravines, from the bottom of which arise the stately Cabbage Palm and the Gigantic Fern, the former from 60 to 90, and the latter from 20 to 30 feet in height, towering amidst the surrounding dark-green foliage, and giving to these secluded spots all the fascination of an Oriental picture.

But the most remarkable feature in the cliffs on this part of the coast is the circumstance of their having been in a state of combustion in three several places; viz. at the points *b*, *c*, and *d*; at the point *b* recently, and at the others evidently at no distant period. The point (*b*) of these cliffs had been frequently observed, by the crews of vessels passing from the port of Newcastle to Sydney, to emit a considerable quantity of smoke. But as it was concluded that the fire, from whence the smoke proceeded, was occasioned by the native blacks, who had, as they term it, "sat down" there, to collect fish from the water left by the receding tide amongst the rocks, no particular notice was taken of the circumstance: and it was only in the early part of the year 1830, that it was accidentally discovered that such was not the case; but that sulphureous vapours, of a very strong and pungent nature, accompanied in some portions of the spot under combustion by brilliant flame, were evolved from several crevices in the cliff, the margins of which were covered with volcanic sal-ammoniac, of different shades of colour; in efflorescent crusts, small botryoidal; and in beautiful crystals intermingled with sulphur. Upon my visiting this spot in the month of August last, I found, however, that the fire was then entirely extinct, some convulsion having evidently taken place, the cliff having separated, and the materials from above having as it were fallen in upon the burning portions, and extinguished the fire. In the same month also, I discovered that the cliff at the point (*c*) had at no distant period been on fire, presenting in its extinguished

state a similar appearance, in its specimens of burnt, yellow and red clay, and white and red clay-stone, to that at the point (*b.*) And in the present month of September, I also found that at the point (*d*) there had been a considerable portion of the cliff under combustion; and large fragments of slag and vitrified clay-stone were picked up on its surface.

The circumstance of these three points *b*, *c*, and *d* being situated each in the immediate vicinity of coal-beds, renders it highly probable that their combustion is to be attributed to a subjacent seam of coal having become ignited, and communicating its flame to the inflammable materials with which they were supplied.

I may here observe, that after a strong south-east gale the shore on this coast is strewed both with small and large rounded fragments of pumice-stone, of four varieties of colour; white, ash-gray, brown, and black. In its texture this pumice bears a striking resemblance to that variety of this mineral which abounds on White Island, aconstantly active volcano, 40 miles to the north of the East Cape of New Zealand; and it may be a question of interest, whether these fragments of pumice are conveyed to these shores from thence, or detached and borne on the waves from some submarine volcano in our more immediate neighbourhood.

The strand on this coast, in many parts composed of sandstone, millstone-grit, and pudding-stone, mixed with ironstone, is singularly divided in several instances into sections resembling rail-roads; and in one spot it is covered with circular masses, much water-worn, in figure resembling those of a curious formation of sandstone discovered by me in Glendon Brook, Hunter's River, and described in a memoir read before the Philosophical Society of Cambridge in the month of March 1830.

Sept. 23rd, 1831.

XXI. *Observations on the House-Spider, in reply to a Statement in the Zoological Journal, quoted in the Phil. Mag. and Annals, vol.* x. *p.* 184. *By* John Blackwall, *Esq. F.L.S. &c.**

NUMEROUS experiments made with the House-spider (*Aranea domestica*), under a great variety of circumstances, have induced me to believe that it is *not* endowed with the instinct to let out lines from the spinners, over which it can escape from captivity, when placed on a twig insulated by water and exposed to a current of air. This opinion, which is published in the Philosophical Magazine and Annals, vol. x.

* Communicated by the Author.

p. 184, has elicited an editorial note having reference to an article in the Zoological Journal, vol. i. p. 283–284, from which, on a hasty perusal, it might be supposed that this instinct is sometimes manifested by the house-spider when so situated. But as it must be conceded, on mature deliberation, that an inference precipitately deduced from a single, and, on the writer's own showing, unsatisfactory experiment, cannot be considered as at all invalidating the conclusion at which I have arrived after a careful and extensive investigation of the subject, I should scarcely have deemed it requisite to bestow a comment on what he has written, had I not felt called upon to do so by the allusion made to it in the above-named scientific publication. Unforeseen obstacles, which it would be tedious to particularize, have concurred to prevent me carrying my intention into effect so early as I could have wished.

In order that the statement of this observer, whose views in more instances than one are opposed to my own, may be duly appreciated, I shall transcribe it at length, offering at the same time such animadversions upon it as a candid examination of its claims to the attention of naturalists has suggested.

"Some years ago," he remarks, "when making some observations on the habits of spiders, I was struck with the following circumstance, which I have never found in any author on the subject. I insulated a common house-spider, by placing it on a little platform, supported by a stick with a weight at the bottom, in the middle of a rummer of water. The platform was about half an inch above the surface, which was nearly even with the top of the glass. It presently made its escape, as was anticipated, by suffering a thread to be wafted to the edge of the glass; but supposing that it might have been assisted by the water being so nearly on the same level, I poured some of it away, and placed the spider as before. It descended by the stick till it reached the water, and examined with its two anterior feet all round, but finding no way to escape, it returned to the platform, and for some time prepared itself by forming a web, with which it loosely enveloped the abdomen, by means of the hinder legs. It then descended, without the least hesitation, into the water, to the bottom; when I observed the whole of the abdomen covered with a web containing a bubble of air, which I presume was intended for respiration, as it evidently included the spiracles. The spider, enveloped in this little diving-bell, endeavoured on every side to make its escape, but in vain, on account of the slipperiness of the glass; and after remaining at the bottom of the water for thirteen minutes, it returned apparently much exhausted, for it immediately coiled itself closely under the little platform, and remained afterwards without motion. This

property of forming for itself a reservoir of air, by means of which it is preserved under water, is somewhat analogous to the interesting habit of the *Argyroneta*, although it serves for a different purpose. In the present case, it is doubtless intended to enable the animal to cross the water with safety."—T.B.

Notwithstanding the boldness of the author's assertion, that the spider, in the experiment cited above, made its escape "by suffering a thread to be wafted to the edge of the glass," it is apparent from his employing the phrase, "as was anticipated," that he had previously received a bias which disqualified him for giving an impartial opinion in a case so defective in evidence as the one under consideration. I say so defective in evidence, because it may be fairly presumed that the escape of the spider was *not* witnessed by the experimenter, who even admits the possibility of the animal having derived assistance from the water, in consequence of its being nearly on a level with the top of the glass. Now the only manner in which the spider could accomplish its purpose by means of the water would be by traversing its surface; and that it actually did so can scarcely be doubted,—for we are informed that when some of the water was poured away, its plan of operations was speedily changed, and every attempt made to regain its liberty proved unavailing.

Additional weight is given to this conclusion by facts which have fallen under my own observation. Various kinds of spiders are known to run upon water with greater facility than they do on land; and though the larger of our indigenous species, including the *Aranea domestica*, are, at least when they have attained their full growth, incapable of doing so, still they sometimes contrive to effect a passage over its surface by the following ingenious expedient. Placed on a twig insulated by water they attach a line to it, which they seize with the foot of one of the hind legs, allowing it to run freely through the claws as it proceeds from the spinners. Descending to the surface of the liquid they use their best endeavours to pass over it; and should a little dust or other extraneous matter happen to rest upon it, enabling them to obtain even a slight footing, their efforts are frequently crowned with success; the line, which chiefly contributes to support them during their progress, and also serves to secure a return to the twig, should their attempts prove abortive, being ultimately made fast to the edge of the vessel containing the water. It is most probable, therefore, that the line which our author affirms was wafted to the edge of the glass, had been conveyed to it in the manner just described.

When spiders descend beneath the surface of water, a bubble

of air is usually seen to envelop the abdomen. Misled by the appearance of the bubble, in the case of the house-spider, which was the subject of his experiment, the author of the article in the Zoological Journal endeavours to account for this fact on the supposition that the air is contained in a web formed about the body of the spider with the assistance of the hinder legs. Those zoologists who are familiar with the external structure of spiders will immediately perceive the insufficiency of this explanation: it will be equally apparent also to the uninitiated, when they are informed, that the cephalothorax and even the legs individually, as well as the abdomen, are frequently encompassed with air. In short, the air, which in the form of bubbles envelops to a greater or less extent the body and limbs of spiders when immersed in water,—instead of being contained in a web, is confined among the hair with which those animals are clothed; consequently, however sudden or unexpected their submersion may be, they are always prepared for it, as I have ascertained by repeated trials.

With these results before us, it will be readily admitted, that in conducting experiments with spiders placed on objects insulated by water, there is a decided advantage in employing, as I uniformly do, vessels having smooth perpendicular sides; care being taken not to *fill* them with the liquid. Moreover, when the experiments are made with hunting spiders, a vessel of considerable internal dimensions should be selected; for if this precaution be neglected, some species, *Salticus scenicus* for example, will escape by leaping over the water intended to confine them; and as on such occasions a line attached by its extremity to the station previously occupied by each individual, is drawn out after it from the spinners, the notion that it had been wafted to the edge of the vessel by a current of air, might be induced in this case, as it was in that of the house-spider, to an exposition of which so large a portion of the present communication is devoted.

XXII. *Particulars of the Measurement, by various Methods, of the Instrumental Error of the Horizon-Sector described in* Phil. Mag. vol. lix. *By* John Nixon, *Esq.**

[Continued from Phil. Mag. and Annals, N.S. vol. x. p. 347.]

By the Sixth Method.

Theory.—THE elevation of a mountain, observed from a station at its base, should be found, on transporting the instrument to its summit, to be equal to the cor-

* Communicated by the Author.

responding depression of the station, both angles being first corrected for curvature and refraction. When the distance is considerable, the uncertainty of the latter element, which sometimes amounts to one minute, and may differ considerably in value at the two places of observation, renders the observations inapplicable to the determination of the constant error of the instrument. In the event of the proximity of the stations being such as to reduce any probable allowance for the refraction to an insignificant quantity, yet would the extreme accuracy required in pointing the telescope and obtaining the measurements of the height of the eye, &c. render the results liable to gross errors. Fortunately, the discovery of Professor Gauss enables us to measure to the greatest accuracy the reciprocal elevation and depression of a ray of light a few inches in length from observations made at its extremities.

The method consists in pointing at each other two telescopes, each furnished with a spirit-level, and marking, when their lines of collimation are parallel to each other, the exact positions of their respective bubbles. Both lines of collimation, it is evident, are now either parallel to the horizon, or are *equally* inclined to it,—with this difference in the latter case, that one is depressed and the other elevated. On pointing a *perfect* telescopic-level first at one and then at the other telescope, (their bubbles being stationary at the marks,) their lines of collimation will appear both level, or one will be found depressed and the other elevated at the same angle. Admitting the telescopic-level to be *imperfect*, half the sum of the observed inclinations, when both are depressions or both elevations, or half their difference, when one is a depression and the other an elevation, will be equal to the instrumental error. When both angles are depressions, or the one of depression exceeds that of elevation, the telescopic-level measures elevations (as in the case of the horizon-sector) in defect.

Apparatus, &c.—The horizontal plank resting on the iron brackets driven into the eastern wall of the room, supported the telescopes, &c. used with the sector in the measurements.

One of the arcs of the sector being placed in a horizontal position with its zero-line coincident with that of its moveable index, hot diluted glue was applied by a hair-pencil to the edge of the vernier part of the index in order to secure it, in default of a clamp, to the arc. After the lapse of a day or two the other index was attached by a similar process to its corresponding arc.

The reversing points of the great levels of the sector were most carefully ascertained, immediately subsequent to the completion of a set of measurements, by numerous observa-

tions made with the instrument steadied on the great stone pillar by three huge discs of lead resting within the lower part of its frame. The reversing point of the right-hand level came out on every trial very nearly the same, but that of the other level proved more fluctuating; the consequence apparently of some accidental injury done to the case. That no displacement of the adjustment of the zero-line of each index to its arc had occurred during the process of measurement was verified by examining through a lens their exact coincidence, and the unbroken appearance of the connecting pellicle of glue.

The upright wire of the sector was rendered perpendicular in the following manner. At the northern extremity of the plank was set up a white board, carrying in the upper part a projecting nail, from which was freely suspended a short plumb-line. At the southern extremity of the plank stood the *round* telescope described page 347*, resting within two substantial Ys glued to a thick board. At the middle of the plank was placed the sector, its eye-tube (about a foot distant from the white board) being drawn out until distinct vision was obtained of the plumb-line as seen by the round telescope through the object-glass of the sector. The upright wire being now made parallel to the plumb-line by moving the cylinder of the sector about within its Ys, the cross levels were adjusted in the usual way †.

Proof Telescopes; and Method of making their Lines of Collimation parallel to each other.

The 30-inch telescope (carrying Fortin's level), which we shall call the *square* telescope, has been fully described at pages 339 and 345.

The other, or *round* telescope just mentioned, was mounted, when one of its cross wires had been rendered truly perpendicular, by the 14-inch level-tube glued to one side of the telescope and also to the Ys. Although the glue, which was extremely dilute, had been most sparingly applied, yet more than a week had elapsed before the position of the level-tube relative to the line of collimation became unquestionably constant.

The square telescope being placed on the plank near its northern end, and a piece of white pasteboard set up a few inches beyond or north of its eye-tube, the round telescope was stationed at the southern end of the plank in a line with

* All the references are to Phil. Mag. and Annals, N.S. vol x.

† This method of setting a wire perpendicular is more convenient, yet certainly less accurate than either of those given at pages 341 and 345.

the other, their object-glasses being nearly in contact. A certain division of the pearl slip of the square telescope, as seen through the round telescope, being made to appear to bisect a dot or minute particle of dust adhering to the vertical wire of the latter, (which was effected by moving gradually in the proper direction a piece of thin paper introduced between the Y stand of the round telescope and the plank beneath it,) the exact position of the level-bubble of each telescope was marked on its tube by a hair-pencil dipped in white paint of the temperature of the room.

Measurement by the Sector of the Inclination of the Lines of Collimation of the Proof Telescopes.

In the first attempt, which brought out the instrumental error 25″, the sector was placed at the south end of the plank, and the round and square telescopes were stationed in succession at the other extremity with a lamp beyond. However, as neither the division of the slip nor the dot on the wire could be satisfactorily bisected by the horizontal wire of the sector, the latter, deprived of its eye-tube, was removed to the north, and white pasteboard set up beyond it in the place of the lamp. One of the proof telescopes, for instance the square one, being placed to the south in a line with the sector, their object-glasses nearly in contact, and the pearl slip apparently bisected longitudinally by the vertical wire of the sector, the bubble of Fortin's level was brought to its mark by the requisite alteration of the inclination of the square telescope on which it was mounted. The horizontal wire of the sector was now made level or in a line with the (horizontal) division of the pearl slip by the rack-work of the sector-stand. This effected, the great level of the sector then uppermost was carefully read off, and the cylinder turned half-round within its Ys, when the wire and division of the slip were again made coincident, and the other great level read off. With these data and the reversing points of the great levels, the inclination of the line of collimation of the square telescope could be readily computed.

The round telescope being substituted for the square one, and moved laterally until its vertical wire appeared a few seconds to the right or left of that of the sector, its inclination was varied until the bubble of its level stood at its marks. In the next place, the horizontal wire of the sector was made to bisect the dot on the vertical wire of the round telescope before and after the cylinder of the sector had been inverted. These operations being followed by the corresponding reading of the great levels of the sector, the inclination of the line of collimation of the round telescope could be obtained.

As an indispensable verification, the two proof telescopes were placed, at the conclusion of the measurements, once more together on the plank with their bubbles at their original marks, when their lines of collimation were found to be parallel, to the accuracy of about a second.

A preliminary trial, unsatisfactory from some uncertainty respecting the reversing point of one of the great levels, made the error 20″. The mean of three succeeding measurements, of which the particulars are subjoined, gives 22″·8, or, if we reject the first set, 22″·3, for the instrumental error. (The discrepancy between the results by the two levels is a consequence of the line of collimation of the sector not being strictly parallel to the axis of the cylinder).

March 19th, 1831. Temp. 51° Fahr.

Depression of (the line of collimation of) the round telescope	by right-hand level	27″·0	Error.
	by left	26·8	= 23″·9
Depression of the square telescope	by right	22·5	
	by left	19·4	

March 21st, 1831. Temp. 57° Fahr.

Depression of the round telescope...............	by right-hand level	26·1	
	by left	28·5	= 22·6
Depression of the square telescope	by right.	16·2	
	by left	20·5	

March 21st, 1831. Temp. 60° Fahr.

Depression of the round telescope...............	by right-hand level	24·5	
	by left..............	27·7	= 22·0
Depression of the square telescope	by right	16·2	
	by left	20·5	

To the above measurements may be added a single set made with the sector placed first on the plank, and afterwards raised by introducing a half-inch board between it and the plank. For want of leisure, the parallelism of the proof telescopes was not verified at the close of the operations; but as it was examined in the interval between the two measurements, no sensible deviation could have possibly taken place in the brief interim. The calculation for the sector on the board comes out 1″ less than with it on the plank, but the sector

wires were not then distinctly seen through the round telescope on account of the smallness of its object-glass. In explanation of the decrement of $2''\cdot5$ in the value of the error ($= 19''\cdot7$), it is to be remarked that the reversing points were obtained at $42°\cdot5$ Fahr., and the measurements at 57° Fahr. Besides which, the reversing point of the right-hand level had diminished $2''\cdot7$, which would affect the constant error by half that quantity *minus*.

March 22nd, 1831. Temp. 57° Fahr.

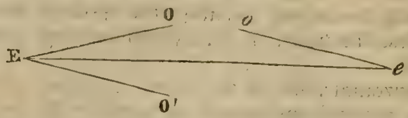
	On Plank.	On Board.
Depression of the round telescope	"	"
by right-hand level	21·2	20·2
by left	26·1	24·5
Depression of the square telescope		
by right-hand level	13·5	13·1
by left	20.0	19·2
Instrumental error............	20·2	19·2

By the Fourth Method.

Theory.—When two telescopes have been pointed in succession from precisely the same place at the same distant object, the position of the bubble of the level, with which each telescope is furnished, being marked, we may ascertain the exact horizontal inclination of the object by placing the two telescopes in a line with their object-glasses opposite each other, and the bubbles of their levels stationary at their marks. Supposing the object to have been exactly on a level with the telescopes, it is evident that on looking through either of them, their lines of collimation would be found parallel to each other. Admitting the object to have been inclined to the horizon, the deviation of parallelism would be equal to twice that inclination, or to the angular equivalent of the displacement of the bubble of either telescope, produced by varying the inclination of the latter, until its line of collimation became parallel to that of the other (*undisturbed*) telescope. It is now clear that when the bubble in question is brought, by a movement of its telescope, to a point of its scale equidistant from its present and original positions, the line of collimation of that telescope will be parallel to the horizon.

Demonstration.—Let E O be the elevation of the line of collimation of one telescope, and *e o* the equal elevation of that of the other. To make the former parallel to the latter, we must depress E O below the horizontal line E*e* by an angle equal to its present elevation above it; its direction being now EO′, and the angle OEO′ the equivalent of the run of the bubble. Then, as this latter angle is bisected by the ho-

rizontal line E c, it follows that when the bubble stands at the intermediate distance, the line of collimation (EO) must be in the direction of and coincident with the line of level E e.



Proof-Telescopes.—These were the square telescope (mounted a year ago with Fortin's level) and another, recently constructed by Dollond. It consists of a firm brass tube, 22 inches long and 1·5 inch in diameter, containing at one end a stiff short tube, or drawer, fitted up with an excellent object-glass, of 12·5 inches focus, and at the other end an inverting eye-piece. At the focus of the object-glass is fixed a stop with a vertical *broad* slip of pearl, which can be moved upwards or downwards by opposite adjusting screws and nuts, until the middle one of three beautifully fine lines drawn horizontally across the slip about the middle of its length is brought to the height required. The telescope rests within a pair of Ys 4·5 inches broad, cut out of a 2·5-inch plank of American birch, which were glued (about a year ago) at a clear distance of 7 inches from each other to a (horizontal) 1·5-inch board (of the same wood) 14 inches long and 6 inches broad. The pearl slip being rendered vertical by moving the telescope about within its Ys until it appeared parallel to the perpendicular wire of the sector, the telescope was glued within the Ys. After a lapse of a day or two, Lealand's level-tube, 14 inches long, having a scale of about $\frac{1}{16}$th of an inch for 2″, was placed, and secured by glue within the brass Ys, soldered, 10 inches asunder, to the upper surface of the telescopic tube.

The square telescope being adjusted to the proper focus by pointing it on a clear day to a steeple at a distance of ten or twelve miles, it was set up on the plank with Dollond's telescope just beyond it, and the distance of the object-glass of the latter from the pearl slip varied until the lines drawn across it appeared, as viewed through the square telescope, at their maximum distinctness. The short drawer containing the object-glass was now secured to the tube at a slit in it, by driving home a broad-headed screw working in the side of the drawer. After the same method the object-glass of the sector was adjusted to the sidereal focus.

The cross levels of the sector were successively adjusted to their marks, when both extremities of the horizontal wire, as measured by the square telescope, had been found, from the

indications of Fortin's level, to be of equal height. It was discovered that the process of adjustment had, however, disturbed the parallelism of the vertical wire to the plane of the arcs; the latter being now a little out of perpendicular, yet by a quantity too minute to affect the result of the measurements, unless the figure of the cylindrical rings proved enormously bad.

The horizontal wire of the sector being scrupulously adjusted, the vertical one served to regulate the pearl slips of the proof-telescopes; effected by making their edges parallel to it. With this precaution, cross levels for the proof-telescopes, otherwise indispensable, were quite superfluous.

To insure the constancy of the adjustment of the zero-line of each index to that of its corresponding arc, Mr. Lealand had recently fitted to each index a powerful clamping apparatus. And it may here be remarked, that the clamping of the indices, the fixing of the levels, &c., were completed many days before the measurements were undertaken, and had passed in the interim through a range of temperature and moisture of great extent.

The trial of the reversing points of the great levels of the sector took place, as soon as possible after every set of measurements, with the instrument standing on the plank, to which it was generally glued, in lieu of placing it, in a different temperature, on the stone pillar. To guard against the slight unsteadiness of the plank, the square telescope was stationed near the sector, the bubble of its highly sensible level being kept stationary between two certain marks of its scale some moments before the reading of the sector levels commenced. The trials, which were very numerous, were not so accordant with each other as might have been anticipated from the extreme care devoted to them. Some of the discordancies were wholly inexplicable; one level continuing constant during the repetitions, and the other oscillating $2''$ or upwards about what proved to be the mean.

Process of Measurement.—The sector* (of which the line of collimation will represent the ray of light from the distant object of our theory,) being placed at the northern end, and Dollond's telescope at the southern end of the plank, the middle line of the pearl slip of the latter was got to be in a line with, or of the height of the horizontal wire of the sector, the vertical wire of the latter apparently bisecting the slip. The level of the sector, then uppermost, and that of Dollond's telescope being both noted, the other telescope was removed, and

* Any other telescope would have equally answered the purpose.

the square one occupied its place. Keeping the sector-level exactly at its mark, a certain division of the pearl slip of the square telescope was made coincident with the horizontal wire of the sector. Fortin's level was then read off.

Substituting for the sector Dollond's telescope, with its level stationary at the previous mark, the division of the narrow slip of the square telescope was got in a line with the middle division of the broader slip of Dollond's telescope, (both slips being longitudinally bisected by the same (imaginary) vertical line,) and the position of Fortin's level subsequently noted.

It is now evident that when (the centre of) the bubble of Fortin's level stood at a point of its scale between, and equidistant from, its last and previous positions, the line of collimation of the square telescope must have been truly horizontal. This level point, as it may be termed, came out,

Feb. 9th (1832), at	124°·5	of Fortin's scale.
10th	124 ·7	ditto.
11th	124 ·5	ditto.

As one degree of the scale answers to $0''{\cdot}6$, the deviation from the mean was limited to a range of $0''{\cdot}1$. Hence the method, from its great simplicity and accuracy, might be successfully applied to astronomical instruments.

Lastly, the square telescope, being still in its place with its bubble at the *level point*, the sector was placed to the north of it (in lieu of Dollond's telescope), and its horizontal wire made coincident with the division of the slip before as well as after the cylinder of the sector had been inverted within its Ys; the reading of the great level uppermost succeeding each coincidence.

With the difference between the reversing point and registered position of each level we compute the observed depression of the (level) line of collimation of the square telescope, which will be equal to the error of the sector.

During the experiments a thermometer, constantly on the plank, ranged between 55° and 57° Fahr.; but a more certain criterion of the uniform temperature of the levels was afforded in the lengths of their bubbles, which occupied constantly the same number of degrees of their scales.

The results of the measurements were,

1832. Feb. 9th.	Temp. 57° F.	Error $= 18''{\cdot}1$	Mean $19''{\cdot}1$
10th.	—— 57	—— $= 20\ {\cdot}1$	
11th.	—— 57	—— $= 19\ {\cdot}1$	

Repetition of the Fourth Method, with the Object-Glass fixed within the Eye-end of the Cylinder.

Being unable to explain why the average of the measure-

ments by the last and preceding methods should exceed by $7''$ those derived from the eleventh method, the eye-piece of the sector was taken out of its cylinder and replaced by the object-glass (described page 339), which was attached to the cylinder by two screws passing through a slit in the latter. By this object-glass the level line of collimation of the square telescope, measured twice, (precisely on the same plan as previously by the proper object-glass of the sector,) appeared under an elevation, not of about $20''$ as might have been anticipated, but of only $5''\cdot 7$. Although the reversing points of the great levels (which were obtained with both object-glasses remaining within the cylinder,) had not varied materially from their preceding value, yet to avoid all risk of error, a set of measurements were immediately undertaken, without the slightest alteration in the state of the sector, by its own object-glass, which gave $21''\cdot 9$ for the error. As the average of the results by the two object-glasses $\left(= \frac{21\cdot 9+5\cdot 7}{2} = 13''\cdot 8,\right)$ agrees within the fraction of a second with that by the eleventh method, it is highly probable that the measurements by the additional object-glass were vitiated either by the tube containing it projecting so much beyond the cylinder as to produce flexure, or because the tube did not fit perfectly tight within it. In fact, when the cylinder was raised carefully out of, but immediately replaced within its Ys, by taking hold of the two tubes containing the object-glasses, the inclination of the line of collimation of the additional object-glass varied several seconds. It should also be remarked that the error by the tenth method, which differs slightly in principle from the eleventh, but does not require an additional object-glass to the sector, indicated the error to be $21''\cdot 3$. On the other hand, it must in candour be admitted that a deflection of the projecting tube occasioned by its own weight would lower the line of collimation of the (adjusted) sector, and tend to augment its constant error.

Professor Bessel states that the pivots of the axis of his meridian circle are cylinders of unequal diameter, and that their axes are not situated in the same straight line. The formulæ for the requisite corrections (*all* the principles of which, I must confess, I do not fully comprehend,) are given in the Phil. Mag. vol. lxiii. pages 434 and 435.

It has been mentioned, (Phil. Mag. and Annals, N.S. vol. x. page 344), that on reversing the cylinder of the sector when fitted up with an extra object-glass placed within the eye-end,

the line of collimation of the latter deviated 11″ in azimuth from an exactly opposite direction. As the cylindrical rings are of unequal diameter, they cannot both come in contact with the same Y at equal heights within it; it would therefore require an almost impracticable symmetry in the sides of the Ys to insure to the axis of the cylinder a precisely opposite direction when reversed within them. With a view to determine whether the deviation proceeded from the irregular figure of the Ys or from the excentricity of the rings, the sector, containing its own and the additional object-glass, was fixed between two proof-telescopes; but the experiments were abandoned on discovering that the line of collimation of the proof object-glass was irremediably so much out of parallel with the axis of the cylinder, that neither the level of Fortin, nor the longer one of Lealand, could measure the angle.

The only consistent theory that can be suggested in explanation is that which assigns a flexure, not to the projecting tube, but solely to the cylinder of the sector. The consequences would be an increased elevation of the lines of collimation of both object-glasses, occasioning an augmentation of the constant error with an object-glass (in its usual situation) at the thicker end of the cylinder, and a diminution of the error when the object-glass made use of is placed at the smaller extremity. Were the cross wires fixed equidistant from the ends of the cylinder, half the sum of the instrumental errors by the two object-glasses (as the increment in one and the decrement in the other produced by flexure would be equal and opposite quantities,) should be equivalent to the error arising solely from the unequal diameter of the cylindrical rings. This half-sum or mean error has been found by the eleventh method to amount to 14″; and the purely cylindrical error comes out by the first method at 17″. Admitting the total error for the proper object-glass to be 20″, its value for the additional one should be $(14 - \overline{20 - 14} =)\ 8''$; which exceeds the actual measurement by only 2″. The cross wires are fixed nearest the smaller end of the cylinder, which would make the flexure most, and the total error least, for the additional object-glass. But were we to confide in the results by the first methods, the flexure for the proper object-glass would be $(20 - 17 =)\ 3''$, and for the other $(17 - 6 =)\ 11''$.

[To be continued.]

XXIII. *On some Atomic Weights.* By Edward Turner, *M.D. F.R.S. Lond. & Ed., Sec. G.S., Professor of Chemistry in the University of London.**

THE adoption by British chemists of the opinion that atomic weights are multiples by whole numbers of the atomic weight of hydrogen, and the experimental contradiction given to that opinion by so distinguished an analyst as Berzelius, induced me about three years ago to undertake an inquiry into the subject. As nearly the sole evidence in proof of the multiple theory is embodied in the First Principles of Chemistry, published by Dr. Thomson, I turned to that work with the view of putting some of the statements, contained in it, to the test of careful experiment. I commenced with investigating the composition of the chloride of barium, because Dr. Thomson had employed it as a means of obtaining a considerable number of his results. My inquiry, published in the Philosophical Transactions for 1829†, proved the existence of a material error; and Dr. Thomson has since acknowledged it by changing the equivalent of barium from 70 to 68 ‡. It is obvious that this error vitiates many of his other equivalents; and that as so great a mistake has been committed in a fundamental question, an inquiry into the accuracy of minor points is superfluous.

I apprehend, therefore, that the atomic weights at present employed by British chemists are unsupported by satisfactory experiments, and that those who adopt the multiple theory cannot adduce exact analyses in defence of the practice. With this feeling I have occupied my leisure for some time past in examining the equivalents of several important substances, endeavouring to ascertain the value of the numbers adopted in this country compared with those of Berzelius. I shall confine myself entirely to results, partly because some of the points are not yet settled to my satisfaction, and partly because I hope early in the ensuing winter to lay the details in a more perfect form before the Royal Society.

Lead.—The equivalent of lead is frequently employed as the basis of calculation in chemistry. The number adopted in this country, on the authority of Dr. Thomson, is 104. Berzelius has lately repeated his earlier experiments on the

* Read before the Chemical Section of the British Association at Oxford, June 27, 1832; and communicated by the Author.

† Dr. Turner's paper "*On the Composition of Chloride of Barium,*" will be found in Phil. Mag. and Annals, N.S. vol. viii. p. 180.—Edit.

‡ Dr. Thomson's correction will be found in Phil. Mag. and Annals, N.S. vol. x. p. 392.—Edit.

subject, by reducing oxide of lead to the state of metal by means of hydrogen gas. Taking his two most widely differing results, the equivalent of lead, oxygen being 8, will be 103·42 in the one case, and 103·64 in the other. His mode of analysis, though apparently easy and simple, is by no means free from practical difficulty. My experiments were made by converting the oxide into sulphate of lead, a method, I believe, susceptible, with the requisite precautions, of greater accuracy than that employed by Berzelius. After many trials I feel certain that the equivalent of lead is not higher than 103·6. It is probably somewhat lower; so that 103·5, nearly the mean of Berzelius's experiments, is very near the truth. This point I hope to clear up by renewed experiments, which are rendered necessary by the extreme difficulty of getting oxide of lead in a state of adequate purity. In the mean time 103·5 is the nearest approximation which experiment justifies: it is useless to go beyond the first decimal, because we are ignorant whether 103·5 is greater or less than the real number.

The following experiment will test the value of this estimate:—If the equivalent of lead be 103·5, then 100 parts of metallic lead should yield 146·38 parts of sulphate of lead. The mean of several closely corresponding experiments by Berzelius is 146·419; and the mean of my own is 146·401. If 104 were the equivalent of lead, 100 parts ought to yield 146·16 of the sulphate,—a number differing widely from the result of experiment, and much beyond the errors of manipulation.

Chlorine.— The most satisfactory experiments I have met with respecting the equivalent of chlorine are those of Berzelius. He obtained from 100 of chlorate of potash 39·15 of oxygen, and 60·85 of chloride of potassium; and found that 100 of chloride of potassium correspond to 192·4 of chloride of silver. According to my own experiments, 100 parts of silver give 132·8 of chloride of silver,—an estimate extremely close to that of Berzelius. From these data it follows that the equivalent of chlorine is 35·45.

To compare with this number the equivalent of chlorine determined in a totally different manner, I prepared some very pure chloride of lead, and separated its chlorine by means of nitrate of silver. From the best experiments I could make, 100 of chloride of lead correspond to 103·24 of chloride of silver. Now, even taking as the equivalent of lead the theoretic number 104, the preceding analysis gives 35·578 as the equivalent of chlorine; and when we take the more correct equivalent of lead 103·5, that of chlorine is 35·45, identical with the number deduced from the experiments of Berzelius. The equivalent of chlorine commonly used by British chemists, namely 36, is therefore erroneous.

I may add that the preceding analysis agrees closely with that of Berzelius; but I prefer my own result, because my chloride of lead appears to have been purer than the specimen employed by him, dissolving in water without the slightest residue. It affords an instructive test of the value of the atomic weights current among us. For, supposing 104, 36, and 110 to be the respective equivalents of lead, chlorine, and silver, it follows that 100 of chloride of lead should yield 104·28 parts of chloride of silver, instead of 103·24 as given by experiment. In fact, as will immediately appear, the equivalent of silver is still more erroneous than those of lead and chlorine.

Silver.—My first attempts to determine the equivalent of silver were by means of the oxide; but different analyses disagreed so widely, that I was obliged to resort to another method. Knowing very nearly the equivalent of chlorine, that of silver may be inferred from the composition of the chloride. According to Dr. Thomson, 100 parts of silver correspond to 132·73, according to Berzelius to between 132·75 and 132·79, and by my experiments to 132·8. The coincidence is very close, and therefore the principal difference in the equivalent of silver will depend on that of chlorine. If 36 be assumed as the equivalent of chlorine, that of silver is 110; and it is 108·08 if 35·45 be chosen as the equivalent of chlorine. An extremely slight difference in the number for chlorine, such as lies entirely within the ordinary limits of error, would raise the equivalent of silver to 108·1 or rather higher, or depress it to 108. While the matter is uncertain it will be most convenient to employ the whole number.

In order, by an independent analysis, to ascertain which of the numbers above mentioned is the more accurate, I prepared some very pure nitrate of silver, kept it for some time in fusion, and converted it into chloride of silver. After repeated experiments, I find that 100 of the chloride of silver corresponds to a quantity of fused nitrate, varying from 118·544 to 118·50. But the theoretic quantity deduced by adopting 110 and 36 to represent silver and chlorine is 117·81, which differs widely from the result of actual experiment; whereas, supposing the equivalent of silver and chlorine to be represented by 108 and 35·45, 100 of chloride of silver should correspond to 118·51 of the fused nitrate. This, then, is additional evidence in favour of the atomic weight of chlorine as above stated.

Nitrogen.—I have endeavoured to ascertain the equivalent of nitrogen by the analysis of the nitrates of silver and lead.

1. From the analysis just stated, I consider 100 of the chloride of silver, containing 75·3012 silver, to be equivalent to 118·5 of

nitrate of silver. Calculating from these elements, and with 108 as the equivalent of silver, we shall find 14·06 as the equivalent of nitrogen. It will be 14·046 if the equivalent of silver be 108·1.

2. As a mean of three closely corresponding analyses, made by converting the nitrate into sulphate of lead, I find that 100 parts of sulphate of lead correspond to 109·307 of nitrate of lead. Calculating the equivalent of nitrogen, on the presumption that 103·5 and 40 are the respective equivalents of lead and sulphuric acid, we shall find it to be 14·101.

Berzelius calculates his equivalent of nitrogen from an analysis of nitrate of lead, and estimates it at 14·18. The difference between us principally depends on a different estimate of the composition of the oxide of lead; and until this point shall be settled with more precision than at present, no certain inference can be deduced from the analysis of the nitrate. I have more confidence in the estimate from nitrate of silver, and feel little doubt that 14 is a very close approximation. Some analyses of nitrate of baryta, but which are not fully in a state for publication, induce me to believe that the real equivalent of nitrogen is nearer 14 than 14·1.

Barium.—From the analysis of chloride of barium, published in my Essay on that compound, no inference could at first be drawn in consequence of the uncertainty respecting the equivalent of chlorine. Now, however, that we have reason to take 35·45 as the equivalent of chlorine, it follows from my analysis that the equivalent of barium is 68·76; and according to the analysis of chloride of barium by Berzelius, it is 68·588. I believe the equivalent of barium is intermediate between 68·6 and 68·8, and in the absence of more exact knowledge 68·7 may be taken as a very good approximation.

The general conclusions which I deduce from the preceding account are the following:

1. The atomic weights commonly used by British chemists have been adopted without due inquiry, and several of the most important ones are erroneous.

2. The hypothesis, that all equivalents are multiples by a whole number of the equivalent of hydrogen, is inconsistent with the present state of chemical knowledge, being at variance with experiment.

3. The subjoined equivalents are very nearly correct:—

Lead	103·5
Silver	108
Barium	68·7
Chlorine	35·45
Nitrogen	14

XXIV. *On a new Membrane in the Eye. By* George Hunsley Fielding, *Member of the Royal College of Surgeons in London, Member of the British Association for the Advancement of Science, Curator of Comparative Anatomy to the Hull Literary and Philosophical Society, &c. &c.**

ACCORDING to the accounts we find in standard anatomical works, we are taught to believe that the Pigmentum nigrum of the eye is placed immediately behind, and in contact with the retina†. We are further taught that the colour of this pigment varies greatly in different animals, and that it is of a very light colour, or even wanting, in the night-prowling animals‡.

Now, the membrane I have discovered is immediately behind, and in contact with the retina: it presents a fine coloured appearance, which varies in different animals; and is of a very light colour in the night-prowling animals. It is evident, therefore, that what I term a membrane, is usually esteemed to be a pigment. To prove that it is a membrane will be the object of the present paper.

1st. What are the nature and properties of the pigment of the eye? It is a mucous substance combined with carbonaceous matter on which its colour depends§; it stains white paper; it is removeable by washing||; no known chemical agent has any power either to alter or remove its colour ¶.

Take a section of a beast's eye from which the whole of the humours and the retina have been carefully removed: you will find a brilliant spot of coloured surface which was evidently immediately behind the retina. The colour will generally be found to be blueish-green, with frequently a tinge of yellow, the circumference round this spot (which spot varies in size, from occupying three-fourths to less than one-fourth of the hollow of the globe,) is of a deep blue.

Now take a piece of white paper and apply it to the blue or green part, you will obtain no stain; wash it with water, you will not remove the colour, nor stain the water:—here then are two easy proofs that its colour is of a different nature from that of the true pigment of the eye.

But it may be said, that what I am describing is the Tapetum; and if so, unless we are to regard tapetum and pigmentum as synonymous terms (which Cuvier, Richerand, John

* Communicated by the Author:—This paper is an abstract of part of one read before the late Meeting of the British Association for the Advancement of Science, at Oxford.

† Bell, Fyfe, Monro, Shaw, &c. &c. ‡ Bell, Fyfe, &c. § Young.
|| Bell. ¶ Bichat.

Hunter, Fyfe, and others seem to do), we shall have two different things occupying the same place, which is impossible. Other anatomists* regard Tapetum and Membrana Ruyschiana as synonymes; and, as Sir Charles Bell justly observes, considerable confusion prevails respecting the precise meaning of the term Tapetum. He, however, defines it to be a pile or fleece laid upon the Membrana Ruyschiana, and states the pigment to be spread upon the tapetum next to the retina, and consequently between it and the retina. It is useless, however, disputing about terms; for all anatomists seem agreed that the pigment is placed immediately behind, and in contact with the retina; and as the part of which I am treating is immediately behind, and in contact with the retina, I apprehend that the following proposition will be the only one I shall have to prove, viz. that the substance placed immediately behind, and in contact with the retina, and known by the name of Pigmentum nigrum, is a membrane, and not a pigment.

1st, We have already seen that it does not stain paper, and is not removeable by washing.—2ndly, Take a section of an eye in which the colours are vivid, place it on the table in a bright light, and fixing your eye on any part, steadily, walk round the table, you will find the colour varies according to the different positions you view it in.—3dly, It presents a bright polished surface, like that of a well-finished mahogany table.—4thly, Carefully detach a small portion of this substance, and put it between two thin pieces of glass; it will present a hard and well-defined outline, and on putting the glasses in closer approximation and suddenly relaxing them, you may perceive the substance expand and contract. Again, view this portion by reflected light, you will perceive its usual colour, but with transmitted light you will have a totally different one. In this point it follows Sir Isaac Newton's laws as regards the colours of thin plates.—5thly, I detached very carefully a small portion of this substance, and placing it between two pieces of fine thin glass subjected it to examination through a fine achromatic Amician microscope by Chevalier; the colour of the portion thus examined, was pale blue by reflected, and reddish-yellow by transmitted light. When placed in the field of the microscope the same change of appearance was observed to take place on viewing it as an opake and as a transparent object. With a power of 800 to the diameter, not only were blood-vessels apparent, but even the globules in those vessels! and by increasing the magnifying power to its utmost extent, the globules appeared of the size of a very small pin's head. I once thought I could trace nervous filaments, but do not

* Shaw and others.

feel quite confident on this head.—6thly, It is possible by chemical agents (which, according to Bichat, have not the slightest effect on the pigmentum of the eye) to destroy and restore these colours at pleasure. Take a section of a beast's eye in which the colours are vivid, and dip it into any dilute acid (nitric, muriatic, or sulphuric), you will perceive the colours immediately begin to fade; now dip the portion in cold water, and on taking it out you will find the colours have disappeared; dip it again into the acid, and the colours will reappear as if by the touch of a magic wand; immerse it again in the water, and they will disappear; and so on as often as you please. The same effect is produced by a solution of ammonia. With a pigment this could not occur; and my impression is, that these beautiful colours depend upon the thickness and disposition of the thin laminæ of which by dissection I can prove this membrane to be composed. The cause of the disappearance and reproduction of the colours by chemical agency, I conceive to be merely the effects of heat and cold upon these thin plates, causing alternate expansion and contraction *.—7thly, The true pigment of the eye will be found in the same eyes which possess this brilliant substance; but it will be found behind this part, and most plentiful on the posterior surface of the choroid in connection with the sclerotic membrane. Thus, in the ox, this substance presents a fine blue tint intermixed with green and yellow, and behind it we have the true pigment, of a rich brown; in the sheep it is very similar; in the deer the bright part is pale blueish-white, the pigment a very light brown; in the cat and fox it is a fine golden yellow, and the true pigment a rich black.—8thly, This membrane (as I must term it) is spread over the whole internal surface of the choroides, next to the Tunica Jacobi or external coat of the retina. It varies in thickness, and consequently in the number of its laminæ. It is thickest where the lightest and most brilliant colours are seen, and thinnest in the circumference where it appears of an intense blue colour, and where, no doubt, the colour of the Membrana Ruyschiana underneath affects it. It is very remarkable that neither the extent of the bright surface presenting these varying colours, nor the tint of the colours themselves, are uniform in animals of the same species.—9thly, Minute injection of the choroid does not affect this membrane.—10thly, I have performed all these experiments on the eyes of the sheep and the ox. As

* Are not these changes of colour more probably referrible to the alterations of texture necessarily induced upon so delicate an organized structure by the application of chemical agents?—Edit.

regards the human eye, I have had very little opportunity for investigation; and though I have proved its existence, I cannot say that it ever presented any distinctly coloured appearance. This, however, will be accounted for when I come, in another paper, to treat of the effects produced by this membrane, on Vision, and to show the necessity that such a membrane should exist. The name I propose to apply as most descriptive of its appearance, is Membrana versicolor.

XXV. *On the Investigation of the Strength of Timber and other Materials, with reference to the recent Experiments and Communications of* Mr. Peter Barlow, Jun. *By* B. Bevan, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

ALLOW me to express my thanks to Mr. Barlow for his additional experiments, and for his candid reply to my observations on his first paper. Experiments of this kind, carefully conducted, are of considerable importance at this time, and will for many years remain so, and tend to add a lasting value to your Magazine.

There are many persons who study the properties of timber and other materials, who have not the opportunity of trying experiments on a proper scale; and those who have the means of doing so, frequently want the disposition, even if they possessed the abilities. Communications of this nature are frequently of more advantage to the practical mechanic than the more abstruse and refined theoretical speculations, which always confer great credit and value on your Magazine. Both have their value; but if we estimate their importance by the number of persons likely to be benefited, those which come within the reach of the practical man will be most valued.

To encourage investigations of this nature, was the object I had in view when I made my first remarks on Mr. Barlow's communication, and not to excite any unpleasant feeling in the author. My suggestion that it would be for the benefit of science to observe as far as could be a uniformity in the specification of the properties of timber and other materials, was not intended to imply any censure upon him for adopting an arbitrary number,—which can readily be reduced to the modulus of elasticity by any mathematician,—but to recommend the use of that more generally adopted specification.

As Mr. Barlow affirms he "does not see what advantage

is gained by considering the weight of the timber, except in the particular case where the question is the deflection of a beam from its own weight," I must therefore be allowed to show that this remark has been rather inadvertently made; and I doubt not that Mr. Barlow will, upon further consideration, see that in almost all buildings and machinery, the weight of the materials used in their construction is a most essential matter of consideration. There may be some few constructions which would be improved by an increase of weight; but generally the value of materials will be estimated in the direct ratio of the strength, and inversely as the weight, when the durability and other qualities remain the same: thus if a new species of wood, equal in durability to oak and teak, could be found of equal strength, but of *half* the specific gravity, would it not be preferred for almost all purposes both in naval and civil architecture, and for machines of almost every description? The same may be said of metals,—the less the weight, and the greater the strength, the more valuable they will be.

If I could construct a crane for moving heavy loads with good Memel timber, at half the expense and of two-thirds the weight of one made of Locust-tree,—should I be excused in adopting the Locust-tree on account of saving two or three cubic feet of wood?

I am ready to admit that there are cases in which space is of importance, as well as strength, but in such cases iron and steel are preferred to wood. It is not my intention to represent that lightness and strength are the only qualities to be attended to, well knowing that other qualities for particular purposes are of importance: but the present discussion does not involve those considerations, but simply the strength, relative to the weight; so that when all other things are alike, the value will be in proportion to the height of the modulus of elasticity.

To render the subject a little more plain, we may calculate the quantity of timber required to form a beam of 20 feet length of bearing, to support a given load, to which a deflection of half an inch may be allowed. This beam, if made of Tonquin Bean, will require about $16\frac{2}{3}$ cubic feet; and if made of Memel timber of the same breadth, will require about $19\frac{3}{4}$ cubic feet to possess the same strength; but the Memel beam will only weigh $\frac{2}{3}$rds of the Tonquin beam, or 370 pounds less, and has therefore the advantage in point of weight. In point of cost, it will depend upon the price per foot of each species. Should the Tonquin beam be 3*s.* $6\frac{1}{2}$*d.*, while the Memel cost 3*s.* per foot, the cost would be alike: except, therefore, this

heavy timber can be procured nearly at the price of Memel, there will be no advantage derived from using it, so far as strength is a matter of consideration. But for its hardness, it may be preferable for blocks and for cabinet work.

Mr. Barlow supposes I have been misled by the small error in his formula, at the head of the sixth column. Being well aware of the principle upon which that column was formed, I made use of the correct formula; and although it may appear strange, it is quite true that I did not discover the error until pointed out by Mr. Barlow in his last paper.

The above remarks are not intended to depreciate the value of Tonquin Bean and the other species of wood in the market, but simply to prove that a species of wood (Memel Deal) which is now supplied in large quantities, will answer all the purposes of the builder. I sincerely hope that whenever opportunity occurs, either to Mr. Barlow or others, they will continue to favour the public with similar valuable information through the medium of your Magazine.

I remain, Gentlemen, yours very truly,

Leighton, July 11, 1832. B. BEVAN.

XXVI. *Inquiry how far the Theory of* M. Elie de Beaumont *concerning the Parallelism of Lines of Elevation of the same Geological Æra, is agreeable to the Phænomena as exhibited in Great Britain. By the Rev.* W. D. CONYBEARE, *M.A. F.R.S. V.P.G.S. Instit. Reg. Soc. Paris.**

THE following remarks were drawn up by the author, in consequence of an inquiry proposed to him by the British Association for the Promotion of Science, at its first meeting at York, in 1831, "how far the theory of M. Elie de Beaumont, concerning the parallelism of the lines of elevation produced by geological convulsions of the same æra, appeared to be confirmed by the phænomena of our own island." This question was referred to Professor Sedgwick and Mr. Conybeare; but circumstances having prevented their communication, the latter alone is responsible for the views contained in the present memoir; although, in the hope that some opportunity of intercourse would have occurred, he may occasionally have used the plural number. He has now only to add his earnest hope that nothing in the following communication will be so misconstrued, as to seem to imply any other feelings than those of the highest respect for the very distinguished talents of M. de Beaumont; for with them, on the contrary, from the period

* Communicated by the Author.

of the short geological excursion he had the pleasure to make in his company during his visit to England, he has ever been most deeply impressed; and on the present subject he regards the views M. de Beaumont has announced, as exhibiting the first attempt to take a generalized and combined survey of some of the most important phænomena which fall within the province of our science, and as one of the most masterly contributions which that science has recently received. But there will always be some danger, when new generalizations first burst on the mind, of their being carried too far; and this danger will be in proportion to the ardour and vigour of the intellect from which they emanate. A fair and candid consideration of conflicting phænomena appears to be the only way of guarding against this danger: the character which Aristotle has given of Plato, "he doubted and investigated," must be that of the sincere lover of philosophical truth in every age.

The sectional researches proposed by the British Association being simply intended to invite discussion, the publication of any materials collected for the purpose remains, of course, with the contributors. The accompanying paper is therefore offered to the Editors of the Philosophical Magazine and Journal of Science, should it suit their pages.

Sully, July 4, 1832.

The question referred to our consideration by the former Meeting of this Society may be thus briefly stated. If we examine the phænomena which appear to have resulted from the action of the causes which have elevated at various geological periods the strata of the earth's crust, especially with reference to the line of direction in which those causes have acted, how far does it appear that these phænomena,—as presented by our own island,—confirm or militate against the hypothesis announced by M. Elie de Beaumont,—as resulting from his observations on the principal continental chains,—that the elevating forces which have acted during the same geological periods have acted in parallel lines of direction; and, *e contra*, that those whose activity must be referred to different epochs have not acted in parallel lines.*

In attempting an answer to this question, it may be observed, that as the conclusions of geological science ordinarily must be deduced from the generalization of very multifarious local details widely scattered, and such as can be collected only by the united and long continued exertions of many independent observers; so it were worse than presumptuous for individuals entering for the first time on a branch of the subject hitherto

* An Extract from M. de Beaumont's exposition of his hypothesis will be found in Phil. Mag. and Annals, N.S. vol. x. p. 241.—Edit.

almost unexplored, to pretend to offer more than a partial and imperfect contribution to its investigation, requiring much extension, and probably many corrections, before it can be considered as having accomplished anything beyond a general tracing out of the line of inquiry to be pursued.

The first point in this inquiry is obviously to determine the geological epochs to which the several elevations we observe should be referred. Now we have direct evidence which can enable us to do this, in very few cases; those, namely, in which, as in the Isle of Wight, we observe the immediate contact of the strata affected by the elevating force, with those which have been unaffected by it; and where moreover these strata also are terms immediately following one another in the regular geological series:—it is evident that this second condition is no less essential to determine the exact geological æra of the disturbance than the first; otherwise, where disturbed and undisturbed strata of remote age are in contact, the disturbing force may have acted during any portion of the long interval which must have elapsed between the deposition of the earlier and later formations: *e. g.* in the Boulogne district at Hardinghen we see the elevated strata of carboniferous strata and coal at the foot of the horizontal strata of chalk; and near Namur, in contact even with the tertiary formations. Now it is evident, that so far as the indications afforded by these localities are concerned, the disturbing forces may have acted at any time between the formation of the carboniferous rocks and the tertiary deposits; and it is only by extending our observations across the transition chains of the Ardennes,—which appear to have been affected by the same disturbances, and which abut on the South against undisturbed horizontal strata of new red sandstone, muschelkalk and keuper,—that we can assign the epoch at the close of the carboniferous period, and anterior to the formation of the new red sandstone, as the probable geological date of the agency of the disturbing force.

In many cases, however, we are not thus able to trace the disturbed district on any of its boundaries in contact with undisturbed beds immediately consecutive in age to some of the disturbed strata; but are reduced to reason from the looser analogy afforded by similar disturbances of the same rocks, but in unconnected geological localities; and it need not be urged that we should be careful not to assign too much importance to conclusions thus obtained.

Again; even as to the convulsions affecting the very same geographical district, it is too much to assume, without distinct evidence, that they have all been produced by one single shock, rather than by a series which may have occurred at

intervals through a long period of ages: thus in the example cited, of the transition rocks of the Ardennes and the coal-fields of the Meuse, it is evident that all the rocks anterior to the new red have been violently convulsed, and those subsequent have been little, if at all thus affected. But who shall say that all this disturbance was produced at one blow? This point, indeed, admits of determination by carefully examining whether a general conformity does or does not pervade the whole of the disturbed series;—for if there be anything like general interruptions in that conformity, every such interruption would clearly indicate a distinct æra of convulsion. Now, *à priori*, it should certainly appear that the idea of a series of successive convulsions seems most conformable to the only analogy presented by actual causes, the operations of volcanic forces; and the careful and minute examinations which would be necessary to ascertain every interruption of conformity in the strata of the disturbed districts have hitherto scarcely in any single instance been accurately made.

Having thus candidly avowed the difficulties and obscurities which hitherto overcloud this important branch of geological inquiry, we may proceed to state the few data on the subject which are as yet to be considered as tolerably ascertained, so far as the geology of this Island is concerned; and in doing this we shall find it most convenient to begin with the convulsions of the most recent order which have been here observed; those, namely, which have occurred during the period of the tertiary formations. The tertiary formations, and the chalk on which they rest, have participated in the general elevation of all the secondary strata of the Island, of which the general line of bearing is from N.E. to S.W., but there is no appearance whatever of this elevation having been the result of any violent sudden or single convulsion; on the contrary, everything indicates that it was a gradual, gentle, and protracted *upheaving* (to borrow a German term), continued without interruption during the whole period of the formation of all these strata; or perhaps some persons may be inclined to refer it rather to an equally progressive depression of the basins of the surrounding ocean: as all the phænomena simply indicate a relative change of level, they will admit an equally ready explanation on either hypothesis. We may observe a very general tendency to parallelism between this line, (although the result of a cause certainly continuing to act in the same direction in the tertiary epoch,) and the earlier and more violent convulsions which we shall hereafter find to have affected the older carboniferous strata before the deposition

of the new red sandstone; for the general line (N.E. S.W.) above indicated, may be more correctly described as a *curve* running nearly N. and S. in the northern part of its course, and trending towards an E. and W. direction towards the South; and in like manner we find the carboniferous lines of elevation generally ranging N. and S. in our northern counties, and E. and W. in the southern. But independently of this *general* elevation, we find in the southern counties three parallel lines of elevation ranging E. and W., and indicative of more abrupt and violent action, which appears to have occurred during the tertiary epoch, and which may very probably be regarded as strictly contemporaneous. The first and most important of these lines of disturbance, is that which having traversed the Isle of Wight, strikes and ranges through the peninsula of Purbeck, and then produces the anticlinal line and parallel faults of the Weymouth district; thus extending over more than sixty miles. It must have produced an angular movement of the strata of many thousand feet, as it has thrown the chalk, plastic clay, and London clay into a vertical position. The section in Alum Bay distinctly exhibiting the contact of the disturbed and undisturbed strata, shows this derangement to have been effected by a single and most violent convulsion, of which the æra is most distinctly marked and precisely limited, being subsequent to the formation of London clay, and anterior to the alternations of fluviatile and marine deposits which characterize the basins of the Isle of Wight and Paris.

II. The anticlinal line of the Weald of Kent and Sussex, ranging from the North of Hastings to the North of Petersfield. —This is the cause of the elevation of the north and south chalky downs, and its disturbing effects may be most strongly traced in the narrow chalky ridge of the Hogsback (in the former), where the strata are considerably inclined: it may very probably be referred to the same æra as the foregoing line of disturbance, to which it is very nearly parallel. We may consider this anticlinal line as prolonged through the chalk by Winchester, and a little north of Salisbury, and thus reaching the Vale of Wardour, which is what Professor Buckland terms a Valley of Elevation: here the Portland limestone is thrown up, and the strata often considerably inclined. On the whole, however, the line now described is rather an anticlinal line of very gentle curvature, than one indicating violent disturbance. It is impossible to dismiss this line without observing how exactly parallel it is to the much older lines of elevation of the transition strata of the Quantock Hills and the Forest of

Exmoor, which, when the eye glances over the map, appear to be its prolongation, and yet are really anterior to the age of the new red sandstone*.

III. A third parallel anticlinal line traverses the Vale of Pewsey, another valley of elevation on the greensand, separating the chalky ranges of Salisbury Plain and Marlborough Downs.—The protrusion of the greensand, in the prolongation of this line at Ham and Kingsclere, (see Buckland's paper †, Geol. Trans. 2nd series, vol. ii.) within the western angle of the London basin, may be referred to the same line of elevation, which will give it an extent of about 30 miles. I am not aware that its effect on the contiguous tertiary strata has been noticed, and can therefore only conjecture that it will probably, on examination, prove exactly contemporaneous with that of the Isle of Wight.

The above elevations, that of the Isle of Wight certainly, and those of the Weald and Vale of Pewsey, by the most probable analogy appear to have taken place subsequently to the formation of the inferior tertiary strata, and before the more recent beds. Elie de Beaumont assigns only the systems of Corsica and Sardinia to this epoch, and characterizes them as having a north and south direction; whereas our examples uniformly range E. and W.

Supplement to I.—Although in the northern portion of our Island the absence of cretaceous and tertiary formations deprive us of this direct test of the æra of the disturbances which have there affected the strata, yet the association of many of these disturbances with apparently the newest varieties of the trap formation, and their intimate analogies, in general direction and in most of their geological circumstances, with those which we trace on the opposite side of a narrow channel, in the basaltic area of Ireland, must at once induce us to refer them to a similar age; and in Ireland this is shown, by the presence of the chalk through which the basaltic eruptions have burst and overflowed, to be posterior to that of the cretaceous formation. On the Scotch coast, in Skye and Mull, we only see the basalt in contact with the oolites and lias, which, as at Portrush, &c. in Ireland, are dislocated, altered, and overflowed by it. But to consider the case more generally, we shall find the general bearing of all the strata in Scotland, as in England, N.E. and S.W., and the same line is protracted into Ireland: this is the general bearing of the southern

* See Geol. Trans. vol. ii. 2nd series, for Dr. Fitton's Hastings Section, and those of the Western Weald, where the anticlinal line ranges through Hastings, and comes north of Petersfield.

† An abstract of this paper will be found in Phil. Mag. vol. lxv. p. 214.—Ed.

transition chain of Scotland, called the Lead Hills, which is continued on the Irish coast by the transition ranges of Down; of the primitive chain of the Grampians, continued in Ireland by the lines of the Derry mountains, &c.; and of the principal undulations of the Grampians, as evidenced by the direction of the great depression which affords a line for the Caledonian canal.

Much of this process of elevation appears to have been like the general elevation of the English strata, gradual and gentle; at the same time that it ranges exactly parallel to many lines of disturbance which have been evidently violent, and produced by sudden convulsions limited to definite single periods. This general elevation clearly continued to act through the tertiary period, because in the Irish portion we see the terminal escarpments of the chalk and of the incumbent ridges of basalt conforming to these general lines. The disturbances effected in the oolitic strata of Scotland, near their contact with the granitic chains of Sutherland, are obviously of indefinite age. We shall notice them, therefore, more at length when speaking of the disturbances generally affecting the oolites, and only mention them here to state that we have no clear evidence which negatives the supposition that they may have taken place even as late as the tertiary period.

Supplement to II. Disturbances during the period between the age of the tertiary formations, and that of the new red sandstone. — Elie de Beaumont has distinguished four different epochs of disturbance during this period: 1. that of the Rhenish system affecting the rothetodte and all the substrata; 2. that of La Vendée and Morvan, to extending the muschelkalk; 3. that of the Erzegebirge, the Côte d'Or and Mount Pilate, including the oolites; and 4. that of the Pyrenees and Appennines, which has also disturbed the cretaceous formations.

Our own island, however, affords us few well marked examples of disturbance during this period, and these scarcely ever afford us sufficient evidence to pronounce on their exact æra; so that we must as yet treat of this part of our subject with a much more vague generality.

In Yorkshire, indeed, in examining the stratification beneath the cretaceous Wolds, we discover that the oolitic series is unconformably arranged, exhibiting a convex curvature and anticlinal line beneath the absolutely horizontal line of junction of the superimposed chalk; but here the curvature is very gentle, and no signs of violent disturbance are exhibited: this anticlinal line appears to range nearly E. and W. As the chalk and its greensands also at the S.W. extremity in Dorsetshire, overlie the edges of the inferior rocks as far as the red marle, we

have here again a want of exact conformity; but the difference is scarcely any where sufficient to be sensible to the eye, and can only be recognised by its results on the grand scale: yet these instances are sufficient to show that the elevating forces have acted somewhat differently in the oolitic and cretaceous systems. In Dorsetshire, indeed, the oolites are affected by considerable disturbances in the vicinity of Weymouth, but these appear to have been connected with the convulsions which overthrew the Isle of Wight in the tertiary period. In Yorkshire, we observe on the coast a considerable dislocation of the alum shale near Cloughton: this point is the more worthy of especial notice, because it is situated in the prolongation of the line of the great Cleaveland basaltic dyke, which extends from the central ridge of carboniferous limestone, and ranging nearly in an easterly direction, intersects the coal-measures, new red sandstone, and even the oolites; so that this point indicates a connexion of the disturbances which have here affected the oolitic system with the convulsions and basaltic dykes of the coal-field. The Northumberland coast near the mouth of the Tyne presents a still more decided evidence to the same effect (so far at least as the magnesian limestone is concerned); for this latter rock is here thrown down by the great 90-fathom dyke, by far the most important of the faults which affect the Newcastle coal-field, inasmuch as it occasionally deranges the level of the strata on either side of it no less than 140 fathoms. It ranges east and west for about ten miles, when it crosses the Tyne; but in the upper part of the valley of the South Tyne, in the prolongation of this line there is an immense fault, called the Stubbick dyke, operating in the same direction, which may therefore be very probably considered as its continuation, and which occasions a long narrow subsided strip of the upper coal-measures to extend transversely across nearly the whole breadth of the mountain limestone chain; so that we must regard this dislocation as one of the most considerable with which we are acquainted. It affects the magnesian limestone not only at Cullercoats, but seven miles further on its course at Killingworth, and the same distance from any other locality to which the magnesian formation now extends. The depression occasioned by the fault becoming here much more considerable (440 fathoms), a small portion of the lower magnesian sandstone or rothetodte here becomes included, as the upper member of the subsided mass of strata:—the inference is clear, that this sandstone formation must at the period when this subsidence took place have extended continuously to this point; and that therefore its removal over a tract at least six miles

in length, must have subsequently been effected by denuding causes of the most violent agency, excepting where a single fragment of it was sheltered from their action by the depression occasioned by this great fault. On the coast south of Cullercoats this same sandstone is traversed by a basaltic dyke: it is true that our evidence as to the date of the convulsions extends only in Northumberland to the magnesian lime, and in Yorkshire to the alum shale; yet the general analogy of the two cases may incline us to consider them as contemporaneous: but the question will still remain, how much younger they may be than the age of the most recent of those rocks which is associated with the inferior oolite. Their direction is nearly, but not exactly parallel, both ranging nearly E. and W.; but the eastern extremity of the main Newcastle dyke inclines a little to the north, and that of the Cleaveland dyke to the south. The general direction of the faults affecting the intermediate (Durham) coal-field is nearly similar; and the circumstances I have mentioned render it very desirable to trace the prolongation of their lines towards the overlying range of magnesian limestone, and to examine how far this rock appears affected by them. In the paper on the magnesian limestone, in the Geol. Trans. 2nd series, vol. iii. some trifling faults affecting this rock in Yorkshire are noticed; but their general direction seems to be at right angles to those now noticed, and parallel to the general elevation of the strata.

The traces of the oolitic formations in Scotland have been much disturbed: those in the islands of Mull and Skye by the eruption of the trap rocks, (as we have already noticed, p. 123,) most probably during the tertiary period. The lines of direction are here very variable: along the coast of Sutherland, near the Brora coal-field (which, as in the eastern Moorlands of York, is associated with the inferior oolite), the lias and oolites come in contact with the granitic mountains, and are much disturbed, the lines of direction being variable, but generally inclining to parallelism with the primitive chains which range N.E. and S.W. Although these disturbances are in juxtaposition to the elevated primitive chains, it would be too hasty an inference to refer them to the protrusion of the granite; the granite may already have assumed its actual position relative to these superstrata, and both the primitive and secondary formations may have appeared together at some later epoch. As we have here no younger formations than the oolites to afford us the means of comparison, we must be unable to pronounce definitively how recently these convulsions may have taken place.

[To be continued.]

XXVII. *Descriptions of several new British Forms amongst the Parasitic Hymenopterous Insects.* By J. O. Westwood, F.L.S. &c.*

Familia Chalcididæ, Westw.

1. *Brachymeria*, Westw. in Steph. Cat. 393. *Chalcis*. Spin.

E *Chalcide typicali* (*Ch. Sispes*) differt corpore obtusiori, antennis brevioribus crassioribus, abdomine subsessili, subconico, vix compresso coxisque posticis brevioribus.—*Chalcis minuta*, Fab.

2. *Pachylarthrus*, Westw., *Pteromalus*, p. Dallm. Sw.Tr.1820.

Caput latum, palpis maxillaribus articulo ultimo maximo inflato; antennæ 13-articulatæ, articulis 3 et 4 annuliformibus, 11–13 clavam parvam formantibus; abdomen ♂ breve subtriangulare.—*Pach. insignis*, Westw. Aureo-viridis, antennis palpisque fulvis, pedibus flavis.

3. *Trigonoderus*, Westw. in Steph. Cat. Mand. p. 396.

Cheiropacho Westw. affine. Thorax subovatus, collare triangulare, antennæ ♀ 13-articulatæ, articulo 2do minuto, 3tio longitudine 1mi dimidio, articulis 4—8 paullò brevioribus æqualibus, ultimis 5 clavam (articulo 8vo paullò majorem) formantibus.—*Tr. princeps*. Westw, Obscurè æneus, thorace posticè aureo nitenti, abdomine aureo-viridi, cyaneo nitenti; antennis nigris basi ferrugineis; alis hyalinis nubilâ elongatâ centrali fuscescente, pedibus ferrugineis, femoribus basi pulvillisque nigris. Exp. alar. 6 lin.

4. *Ormyrus*, Westw.

Antennæ breviores crassæ ut in *Cheiropacho* formatæ. Thorax convexus; abdomen ♀ cylindrico-convexum apice conicum, thoracis latitudine et illo paullò longius, segmentis 2—5 hirsutis punctatis et in singuli disco, serie transversò impressionum denticulatarum ornatis. Oviductus breviter exsertus.—*Orm. punctiger*, Westw. Aureo-viridis, abdomine cupreo parùm nitente. Antennæ nigræ, apice fuscæ; scutellum nitidissimum; pedes nigro-virides, tibiis anticis geniculisque posticis obscurè ferrugineis; tarsi pallidi; alæ vix fulvescentes.

5. *Theocolax*, Westw.

Apterus. Caput subhorizontale subquadratum planum, antice minimè tridentatum. Antennæ mediocres, 11-articulatæ; articulo 2do majori articulis 3—8 sensim crassioribus, ultimis tribus clavam, articulo priori (8vo) majorem formantibus. Collare magnum triangulare. Abdomen oviductu breviter exserto.—*Th. formiciformis*, Westw. Fulvo-fuscescens, abdomine obscuriori.

6. *Macroglenes*, Westw.

Caput latum, oculis partem ejus majorem occupantibus, antennæ breves, apicibus crassis, 10-articulatæ articulo 2do mediocri, 3—5 minutis, 6to magnitudine 2di, 7mo præcedenti majori; ultimis tribus clavam magnam formantibus, abdomen compressum.—*Macr. oculatus*, Westw. Atro-cærulea oculis rubris aut piceis tarsisque pallidis.

7. *Cerchysius*, Westw. *Encyrtus*, p. Dallm. Curt.

Tibiæ intermediæ alarumque nervi *Encyrti*. Antennæ ♀ cylindricæ, apice paullò crassiores, 10-articulatæ, articulis 2—7 subæqualibus; ultimis

* Communicated by the Author.

tribus clavam compressam formantibus apice obtuso; abdomen oviductu valido exserto, abdominis fere longitudine.—*Encyrtus urocerus*, Dallm. Sw. Trans. 1820. p. 368.—Sp. 2. *Cerch. stigmaticalis*, Westw. Cæruleo-viridis abdomine cyaneo, antennarum flagello oviductuque omninò nigris; alis fasciâ paullò ante medium pallidè fuscescente, stigmate obscuriori ramulo stigmaticali fusco.

8. *Cirrospilus*, Westw.

Eulopho affinis, et plùs minùsve fulvo variegatus. Caput anticè, inter oculos, emarginatum; antennæ ♀ breves crassæ 7-articulatæ, articulo 2do 3tii dimidio longitudine, hoc 4to longiori, ultimis tribus clavam, articulo 4to vix crassiorem, formantibus. Abdomen (petiolo brevi distincto) depressum ovatum, posticè conicum.—*Cirr. elegantissimus*, Westw. Caput, thorax, pedesque pallidè flavescentes, oculis capitisque vertice nigris; lineâque irregulari per medium thoracis currenti, anticè posticèque dilatatâ, nigrâ. Abdomen fulvum, maculâ centrali irregulari-quadratâ alterâque posticâ subtrigonâ, nigris. Antennæ fuscæ.

9. *Euplectrus*, Westw.

Eulopho affinis. Caput parvum; antennæ graciles 9-articulatæ articulo 2do breviori, articulis 3—6 ovatis, ultimis 3 clavam (vix articulo 6to majorem) formantibus. Thorax ovato-circularis anticè subacuminatus. Abdomen (petiolo brevi distincto) thorace majus, circulare, spatuliforme, depressum. Coxæ posticæ permagnæ tibiæque posticæ calcari longo instructæ.—*Eupl. maculiventris*, Westw. Capite thoraceque nigris abdomine fulvo, lateribus anticis fasciisque transversis apicalibus fuscis. Antennæ, os et pedes, fulvi.

10. *Dicladocerus*, Westw.

Eulopho typicali (*Eul. ramicornis*) differt antennis ♂ tantùm biramosis, sc. 9-articulatis articulo 2do parvo, 3tio 4toque longioribus, horum singulo ramum elongatum e basi emittente; 5to 6toque crassioribus simplicibus, ultimis 3 clavam brevem formantibus.—*Dicl. Westwoodii*, Steph. Cat. 397. No. 5501. Caput thoraxque purpurei viridique nitentes, abdomine cyaneo-nigro, basi lateribus æneis, antennis cyaneo-nigris, pedibus æneo-nigris geniculis articulisque tarsorum basalibus pallidis.

Familia PROCTOTRUPIDÆ.

11. *Platymischus*, Westw.

Apterus, depressus, angustus. Caput subquadratum anticè subacuminatum. Antennæ 14-articulatæ, articulo 1mo maximo, subtriangulari; 2do parvo; 3tio illo majori, internè producto; articulis 10 sequentibus subæqualibus, filiformibus; 14mo paullò longiori. Thorax oblongo-quadratus. Femora incrassata. Tarsi antici articulo 1mo dilatato; abdomen ferè thoracis magnitudine, segmento 1mo maximo.—*Pl. dilatatus*. Steph. Cat. Mand. p. 399. Niger, nitidus, thorace posticè villoso, antennarum articulis tribus basalibus pedibusque rufescentibus.

12. *Megaspilus*, Westw. in Steph. Cat. Mand. 400. *Ceraphoron*, Latr. Curt. et Jurine?

E *Ceraphrone typicali* (*Cer. sulcatus*, Jur.) differt, alis superis nervo costali incrassato, stigmate maximo suborbiculari vel semicirculari, cellulâ apicali unicâ incompletâ, ramo arcuato formatâ; antennisque fractis et in utroque sexu 10-articulatis, apice in ♀ vix vel minimè incrassato.—*Cer. Dux*, Curt. Brit. Ent. pl. 249. f. 1. 3. 4. 5. 8, ♂. 1a. ♀.

13. *Paramesius*, Westw.

Cineto genuino affinis. Caput subquadratum tuberculo antico; antennæ ♂ corpore toto longiores, graciles, filiformes, 13-articulatæ, articulis longitudine subæqualibus (2do 3tioque minutis exceptis) articulo 4to ad basin minimè exciso; abdomen elongato-clavatum, petiolo tertiam partem longitudine æquante, alarum nervi ut in *Cineto gracilipede* (Curt. Brit. Ent. 380. fig. 9.) at areola marginalis paullò longior et basi truncata est.—*Par. rufipes*, Westw. Niger, nitidus, antennis fuscis, pedibus rufis.

14. *Aneurhynchus*, Westw.

Galeso affinis. Caput transversum tuberculo brevi antico, trophis brevibus, antennæ ♂ vix corporis longitudine, filiformes, 14-articulatæ, articulo 1mo simplici, 2do minuto, 3tio tenui, et paullò longiori, 4to crassiori, et ad basin externè minimè exciso. Alæ stigmate nullo distincto, sed nervo subcostali basali, cujus apex alarum marginem anticum non attinet sed obliquè in alarum disco breviter protenditur, indè ad alarum apicem reflectitur areolam marginalem elongatam efformante, nervi reliqui ut in *Cineto gracilipede.*—*An. galesiformis*, Westw. Niger nitidus, antennarum articulo 2do pedibusque rufo-piceis, femoribus basi obscurioribus, alis pallidè fuscescentibus.

15. *Spilomicrus*, Westw.

Subgenus *Diapriam* cum *Galeso* connectens. Caput transverso-quadratum. Antennæ ♀ capite thoraceque paullò longiores, 13-articulatæ, ad apicem sensim incrassatæ; alæ stigmate parvo ante medium alarum, quadrato, apice internè deflexo, ramulum parvum, versus basin alarum reflexum, emittente; areola basali subtriangulari; nervi reliqui ferè ut in *Paramesio*, at indistinctissimi. Metathorax utrinque posticè spinosus. Femora clavata, pedunculus abdominis mediocris, striatus.—*Spil. stigmaticalis*, Westw. Niger nitidus, pedibus obscurè piceis, alis pallidè flavescenti-fuscis, stigmate nigro.

16. *Epyris*, Westw.

Bethyllo affine. Caput mediocre subconvexum; antennæ elongatæ filiformes 13-articulatæ, articulo singulo cylindrico nec ad basin tenuiori. Thorax elongato-ovatus. Metathorax suprà longitudinaliter 3-carinatum. Alæ areolâ unicâ apicali longiori incompletâ areolisque duabus basalibus longitudine æqualibus.—*Epyr. niger*, Westw. Niger, abdomine nitido, tibiis tarsisque plùs minùsve piceis.

XXVIII. *Notice of some recent Magnetical Discoveries. By* M. A. Kupffer, *of the Imperial Academy of St. Petersburg; in a Letter to* Sir David Brewster, *K.H. LL.D. &c.*

By means of a number of experiments continued during the greater part of the winter of 1831, I have found that the intensity of the magnetic forces, in bars of steel, is diminished as much by the action of cold as by that of heat: I speak here of that part of the magnetic intensity which is lost when we expose a magnetized bar to a temperature higher than any which it has experienced since it was magnetized, and which is no longer found after cooling. I have hence adopted a more satisfactory method to procure magnetized cylinders of

a constant force, for measuring the intensity of the earth's magnetism. I not only plunge them several times in boiling water, but I cool them as often down to −20° or −25° of Reaumur, which is not difficult in our climate. This method has succeeded so perfectly, that I can recommend it to scientific travellers.

I have also established the existence of a daily variation in the inclination of the needle and in the magnetic intensity, by direct methods; that is to say, by observing every day the march and duration of the oscillations of a dipping-needle, very long, and suspended on a knife-edge. I have found *that the inclination is several minutes greater at* 11 *o'clock in the morning than at* 11 *o'clock in the evening. The intensity, on the contrary, is greater in the evening than in the morning.*

XXIX. *Account of the Magnetical and Meteorological Observations made at Pekin, by M.* George Fuss. *Communicated in a Letter from* M. A. Kupffer, *of the Imperial Academy of St. Petersburg, to* Sir David Brewster, *K.H. LL.D. &c.*

M. FUSS, the perpetual Secretary of the Academy of St. Petersburg, has just communicated to me a letter which has been addressed to him from Pekin by his brother, who is at present with the Mission which the Russian Government sends out every ten years. At my request the Academy of St. Petersburg furnished M. Fuss (who set out from this place in the spring of 1830,) with all the instruments necessary for making magnetical observations. He has with him two declination needles, one of which was executed by M. Gambey of Paris, and which will serve also for observing the hourly variations of declination; and these needles will remain at Pekin after M. Fuss's return to Russia, about the end of the present year. M. Fuss has also a dipping-needle, which is also from the workshop of M. Gambey;—several magnetic cylinders for observing the intensity, and a chronometer, besides the instruments for astronomical observations. The magnetical observations will be continued at Pekin, after M. Fuss's departure, by M. Kowanko, officer of mines, who will continue there during ten consecutive years. I send you an extract from this letter, and beg that you will communicate it to the Royal Society of Edinburgh*, and insert it in your Journal.

* The sittings of the Royal Society of Edinburgh were concluded before the arrival of this letter.

Letter from M. George Fuss *to his Brother at St. Petersburg.*

"Pekin, April 22, 1831.

"In spite of the numerous obstacles which presented themselves during my journey from Kiankso to Pekin,—both from the difficulties of the road, and from a distrust of our Chinese escort,—I have been able to determine at *seventeen* points, the inclination and the magnetic intensities; and at *eight* points the declination and the latitude. The longitudes have not been determined by the precise methods which were particularly recommended in my instructions (the transits of the moon and the occultation of stars); for the erection of the transit instrument and the great telescope would have excited too much the attention of the Chinese, and awakened their distrust. I hope, however, that in returning I shall be less embarrassed, and that I may then be able to occupy myself more successfully with the exact determination of the geographical position of some important points. At Dyan-dsia-keou, (Khalgan,) however, I have observed for the longitude the occultation of a small star in Capricorn, of the seventh magnitude, by the moon.

"Soon after our arrival at Pekin, there was constructed, at my request, in the garden of the Mission, a column of masonry for astronomical observations. A tent, of a particular construction and very commodious, sheltered the observer from the wind and the weather. The only inconvenience of this locality is, that the horizon is covered almost all round by adjacent houses. The cross of the Church of the Mission, which is distant from my little observatory only about ten toises, serves as a mark for the declination needle.

"Though this distance is not very great, I have however obtained a very satisfactory agreement among my observations, after having cut small cavities for receiving the screws of the needle in the plate of marble which covers the column, and upon which the instrument is placed. The declination needle of M. Gambey gave me, on the 10th of January, 1831, at Pekin at 3^h P.M., a declination of $1° 42' 57''$ W. The dipping-needle of Gambey gave, on the 30th of December, 1830, a dip of $54° 52'{\cdot}1$, which is a mean between the results obtained by two different needles. The method of arbitrary azimuths* gave me, on the 6th of April, $54° 50'{\cdot}7$. It is proper to remark here, that the Chinese do not employ iron in the construction of their houses. I have also observed the horary variations of declination during the winter solstice, and during

* An account of this method will be found in my Memoir on the Dip at St. Petersburg, inserted in Poggendorf's Annals, Observation 1.—*Note by M. Kupffer.*

the spring equinox, on the same days and at the same hours at which M. Kupffer observed at St. Petersburg. I have also determined the intensity of the terrestrial magnetic forces at Pekin, and at other points of my journey.

"Relative to the geographical position of Pekin, I have observed, 1st, eleven transits of the moon by the transit instrument; 2ndly, a central eclipse of α Tauri by the moon, with the great telescope of Dollond; 3rdly, during twelve days from the winter solstice to the present time, the height of the sun at noon for the determination of the latitude, which I have found to be nearly $39° 54' 9''$; and, 4thly, ten times, the transits of different stars across the plane of the prime vertical, to deduce the latitude according to the method of Bessel.

"I have observed also since my arrival, four times a day, the state of the barometer and thermometer. The greatest barometric height of 345·7 French lines took place on the 8th of March at midnight; and I am informed that on the same day, in the northern provinces, there was felt an earthquake. The smallest barometric height took place on the 20th of April, at six in the evening: it was 330·9 lines, and it was followed by a tempest. The greatest heat which has yet taken place was on the 20th of April, at 4^h P.M.: it was 25° cent. The greatest cold was 13° cent.: it took place on the 5th of February, at 6^h A.M. In the same month, however, on the 17th, the temperature rose even to 10°·5 cent. The cold was constant during the second half of the month of January: in the other half, as in the month of December and the beginning of the month of February, the temperature oscillated round the point of the congelation of water; since the 13th of March it has been constantly warm. A barometer and thermometer will remain at Pekin, which will be observed during the ten years that the Mission will remain in China*."

XXX. *Note on the Mean Temperature of Nicolaieff, as deduced from the Observations of* M. Coumani. *By Professor* M. A. Kupffer, *of the Imperial Academy of St. Petersburg.*†

M. COUMANI, at Nicolaieff, has communicated at different times to the Academy of Sciences at St. Petersburg, meteorological observations, carried on by himself, and through his means, with a perseverance, well worthy of imitation, at Nicolaieff and Sevastopol. These observations are reduced with much order, and to the register of each month is annexed a very elegant graphical view of the results.

* All the dates in this letter are reckoned by the New Style.

† Communicated by the Author.

Nicolaieff is situated on the Black Sea, in north lat. 46°58′ ½, and in longitude 32° 0′ east of Greenwich; and its height above the Black Sea is about 20 toises.

The following are the mean results of these observations; and it must be observed that they are reckoned according to the Julian Calendar, which is still employed throughout all Russia. The observations were made twice a day at 10ʰ A.M. and 10ʰ P.M., and the maxima and minima were also observed.

TABLE I. *Mean State of the Octogesimal or Reaumur's Thermometer at Nicolaieff, in the Years* 1827—1830.

	1827.		1828.		1829.		1830.	
	Mean of 10ʰ A.M. and 10ʰ P.M.	Mean of Maxim. and Minim.	Mean of 10ʰ A.M. and 10ʰ P.M.	Mean of Maxim. and Minim.	Mean of 10ʰ A.M. and 10ʰ P.M.	Mean of Maxim. and Minim.	Mean of 10ʰ A.M. and 10ʰ P.M.	Mean of Maxim. and Minim.
January	+ 0°·3	+ 0°·5	− 7°·8	− 7°·7	− 7°·6	− 6°·0	− 7°·8	− 7°·5
February	− 0·5	0·8	− 1·1	− 1·4	− 1·2	− 0·5	− 2·7	− 2·5
March	+ 5·3	5·6	+ 5·2	+ 5·3	+ 4·9	+ 4·8	+ 2·8	+ 3·0
April	9·9	10·6	15·1	11·0	10·9	11·0	9·9	9·6
May	16·6	16·9	14·5	14·5	13·2	13·2	15·6	15·7
June	19·3	19·7	19·3	19·1	16·2	16·9	16·8	16·8
July	19·9	20·5	18·7	18·9	18·2	18·4	18·3	18·1
August	15·7	17·0	16·3	16·8	16·2	16·6	17·3	17·3
Septemb.	11·4	11·6	10·9	10·9	13·6	13·7	10·9	10·9
October	6·3	6·4	3·4	3·4	+ 3·9	+ 4·1	4·8	5·1
Novemb.	+ 1·1	+ 1·0	+ 0·4	+ 0·5	− 4·1	− 4·2	3·3	3·3
Decemb.	− 2·6	− 2·6	− 6·4	− 4·5	− 4·1	− 3·8	+ 1·6	+ 1·7
Means	+ 8 5	+ 9·0	+ 7·4	+ 7·2	+ 6·6	+ 7·0	+ 7·6	+ 7·6

	Reaumur.	Fahr.
Mean Temp. of 1827—1830, at 10ʰ A.M. and 10ʰ P.M.	+7°·52	48°·92
Mean Temp. at the hours of Max. and Min....................	+7 ·70	49 ·32

TABLE II. *Mean Temperature of Wells at Nicolaieff, in* 1827, 1829, *and* 1830.

	1827.	1829.		1827.	1829.
January ...	+6°·2	+2°·5	July.........	+9°·0	+8°·8
February...	6·1	2·1	August ...	8·7	8·8
March	6·8	4·9	September	8·3	8·8
April	7·8	7·2	October ...	7·5	7·3
May.........	8·6	7·7	November	5·9	4·5
June	+9·0	+8·6	December	+4·6	+3·1

Mean of 1827 .... 7°·4
Mean of 1829 .... 6 ·2

In 1828 the observations on the temperature of this well were interrupted; but they were resumed in 1829, and observations were also made on another well.

A spring at Nicolaieff gave in 1830 the following results:

January...	+9°·6	April......	+9°·4	July	+9°·4	October...	+9°·5
February	9·5	May	9·5	August ...	9·4	November	9·6
March ...	+9·3	June	+9·4	September	+9·5	December	+9·6

Mean...... 9°·5 Reaumur...... 53°·38

TABLE III. *Extreme Variations of the Octogesimal Thermometer at Nicolaieff, in each of the Months of the Years* 1827—1830.

	1827.			1828.		
	Max.	Min.	Diff.	Max.	Min.	Diff.
January......	+ 7°·6	−10·5	18°·1	+ 4°·2	−24°·5	28°·7
February ...	11·0	11·0	22·0	8·5	15·0	23·5
March	12·7	− 3·5	16·2	15·9	− 4·6	20·5
April.........	21·6	+ 1·7	19·9	20·9	+ 2·0	18·9
May	27·0	7·0	20·0	24·0	4·5	19·5
June	30·0	12·5	17·5	27·2	8·0	19·2
July	29·5	11·0	18·5	27·3	10·0	17·3
August	29·0	5·7	24·3	28·1	5·0	23·1
September...	20·5	+ 2·8	17·7	21·6	+ 2·5	19·1
October......	18·3	− 2·0	20·3	14·1	− 2·0	16·1
November...	10·4	10·5	20·9	8·0	12·6	20·6
December...	+ 4·0	−14·0	18·0	+ 8·0	−13·9	18·9

	1829.			1830.		
	Max.	Min.	Diff.	Max.	Min.	Diff.
January......	+ 2°·0	−20°·2	22°·2	+ 3°·4	−21°·0	24°·4
February ...	6·5	11·7	18·2	5·1	12·0	17·1
March	15·0	− 3·2	21·2	12·2	3·0	15·2
April.........	20·4	+ 2·0	18·4	21·2	− 1·0	22·2
May	25·0	4·7	20·3	27·5	+ 7·0	20·5
June	25·5	9·6	15·9	25·0	9·5	15·5
July	29·0	8·9	20·1	28·5	8·4	20·1
August	24·5	8·0	16·5	28·6	7·7	20·9
September...	21·0	+ 6·0	15·0	20·5	+ 0·5	20·0
October......	16·3	− 3·3	19·6	15·2	− 3·0	18·2
November...	8·9	17·5	26·4	10·0	2·7	12·7
December...	+ 4·6	−19·5	24·1	+ 9·5	−11·0	20·7

TABLE IV. *Winds which blow at Nicolaieff during the greatest Heat and greatest Cold of each Month.*

	Winds during Max. Temp.		Winds during Min. Temp.			Winds during Max. Temp.		Winds during Min. Temp.	
	1828.	1830.	1828.	1830.		1828.	1830.	1828.	1830.
January	SW	NE	NNE	NNW	July ...	SE	SSE	WSW	
Feb. ...	SSE	SW	NNW	NW	August	WNW	NNE	NNE	NNW
March	SW	SSE	W	NNW	Sept. ...	SSE	SW	NW	SW
April ...	SSE	SE	NW	N	October	WNW	SW	N	W
May ...	NE	WSW	NW	Calm	Nov. ...	SSE	SW	N	N
June ...	SW	SSW	NE	NW	Dec. ..	SW	SSE	N	N

TABLE V. *Greatest Variation of the Octogesimal Thermometer at Nicolaieff, on the different Days of each Month.*

	Greatest Variation in a Day.					Greatest Variation in a Day.			
	1827.	1828.	1829.	1830.		1827.	1828.	1829.	1830.
	°	°	°	°		°	°	°	°
Jan. ...	8·0	13·1	10·2	9·9	July ...	11·8	15·5	11·4	13·5
Feb. ...	14·3	8·8	10·7	9·5	August	9·7	13·1	12·3	13·5
March	10·1	10·9	12·3	11·2	Sept. ...	10·8	13·7	12·4	12·0
April ...	14·9	12·9	11·9	14·2	Oct. ...	13·5	9·1	9·8	11·7
May ...	10·0	11·5	13·1	15·0	Nov. ...	7·0	9·2	7·9	5·2
June ...	14·0	11·2	13·0	11·5	Dec. ...	7·7	8·7	7·9	7·3

TABLE VI. *The Means of the Maxima and Minima of each Day, for every Month of the Year.*

	1828.			1829.			1830.			
	Mean of Max.	Mean of Min.	Diff.	Mean of Max.	Mean of Min.	Diff.	Mean of Max.	Mean of Min.	Diff.	Mean of the Diff. for the Three Years
	°	°	°	°	°	°	°	°	°	°
Jan. ...				− 4·9	− 7·0	2·1	− 4·9	−10·1	5·2	3·7
Feb. ...	+ 0·9	+ 1·4	2·3	+ 1·9	− 2·9	4·8	+ 0·5	− 5·4	5·9	4·3
March	8·1	2·4	5·7	8·0	+ 1·5	6·5	5·6	+ 0·5	5·1	5·4
April ...	15·7	6·3	9·4	15·4	6·5	8·9	13·9	5·2	8·7	9·0
May ...	19·0	10·1	8·9	17·3	9·0	8·3	20·5	10·8	9·7	9·0
June ...	23·6	14·6	9·0	21·1	12·7	8·4	21·1	12·5	8·6	8·7
July ...	23·4	14·0	9·4	22·8	14·0	8·8	23·0	13·3	9·7	9·3
August	21·5	12·1	9·4	21·3	11·9	9·4	21·7	12·7	9·0	9·3
Sept. ...	15·1	6·8	8·3	18·2	9·1	9·1	15·4	6·7	8·7	8·7
Oct. ...	5·2	+ 1·7	3·5	+ 6·8	+ 1·3	5·5	8·4	1·7	6·7	5·2
Nov. ...	+ 2·4	− 1·4	3·8	− 2·0	− 6·4	4·4	4·5	+ 2·0	2·5	3·6
Dec. ...	− 2·3	− 6·7	4·4	− 1·4	− 6·2	4·8	+ 3·6	− 0·2	3·8	4·3

[*Observations on the preceding Results.*

In a paper on the Mean Temperature of the Earth, published in the 9th volume of the Edinburgh Transactions, and also in the Edinburgh Journal of Science, No. VIII. p. 300, I have shown that the temperature of the globe is distributed in reference to two axes different from the axis of rotation; and I have constructed formulæ, founded on this principle, for computing the mean temperature at any point of the earth's surface. In order to compare this theory with observations, especially round the Asiatic Pole of maximum cold, it became desirable to have accurate observations on the mean temperature of various points in the interior of the Russian empire. Professor Hansteen, previous to setting out on his journey to Siberia, kindly undertook to procure for me such observations; and for the same purpose Professor Kupffer, of St. Petersburg, has had the kindness to send me several valuable

sets of reduced observations, which are of the highest value in reference to this important branch of meteorology. These observations will be published in successive Numbers of this Journal; and while they will enable me to compare my own theoretical view with observations, they will be received by the scientific meteorologist as data of inestimable value in fixing the principles of this new science.

It appears from the first of the preceding tables, that the mean temperature of Nicolaieff for four successive years, from 1827—1830, at 10^h A.M. and 10^h P.M. is $7°·52$ Reaumur, or $48°·92$ Fahr. When we correct this result by $+ 0·122$, the quantity by which the mean of 10^h and 10^h differs from that of the 24 hours, we obtain,

	Fahr.
Corrected mean temp. of Nicolaieff	49°·04
Add for elevation of 20 toises..........................	·36
Mean temp. of level of sea	49°·40
Mean temp. calculated by formula $T = 86·3$ sin. $D - 3°\frac{1}{2} D$, the dist. from the Asiatic Pole being $= 39°·27$	51 ·33
Difference between the observation and the formula	$+1°·93$
The mean temperature of the year 1827 at Nicolaieff was fully	$52°\frac{1}{4}$

So that the formula gives a result within the varying limits of observations for different years, and differing very little from the mean result. D. B.]

XXXI. *On the Refraction of differently-coloured Rays in Crystals, with one and two Axes of Double Refraction. By* M. Rudberg.

[Continued from page 6.]

Section II. *Refraction in Crystals with two Optical Axes.*

THE crystals of this kind, which I have been able to procure, were arragonite, colourless topaz, and the topaz of Schneckenstein. I have not, however, been able to make use of the last of these, of which I have large and fine specimens, because throughout their interior there are cleavage planes, which being always parallel to the external planes reflect the sun's rays in so confused a manner, that the spectrum is not distinct. I have consequently been able to make experiments only with arragonite, and white or colourless topaz.

Before giving an account of these experiments, I shall briefly explain the theory of double refraction in crystals with two

axes, because it is only by this elegant theory of Fresnel that we can find the directions in which the prisms must be cut. Fresnel founded his theory on two hypotheses, viz. 1. That in doubly refracting crystals the elasticity of the vibrating medium is different in different directions; and 2. That the vibrations of the light polarized are at the same time perpendicular to the direction of its propagation and to the plane of polarization.

He supposes that in every crystallized substance there are three directions perpendicular to each other, called axes of elasticity, according to which the elasticity may in general be different. If the elasticity is the same in all these three directions, the crystal belongs to the regular system, and has no double refraction. If it is equal in two directions, the crystal refracts doubly, and has *one* optical axis; and if the elasticity is unequal in all the three directions, the crystal has *two* optical axes. From this difference of elasticity there results for light a different velocity, which ought necessarily, in general, to become unequal for the two rays into which the light becomes divided itself, and whose planes of polarization are perpendicular to each other. There are in crystals with two optical axes only two directions; that of the axes themselves, in which the two rays are propagated with the same velocity. Consequently, in order to appreciate the velocity of the two rays in any direction, we must determine their planes of polarization, which is done by the following considerations. The plane in which the two optical axes are situated contains also two of the axes of crystallization, one of which bisects the acute, and the other the obtuse angle of the optical axes. If we conceive, then, two planes passing through the direction in which we wish to have the velocity of the two rays, and respectively through each of the optical axes, the plane which bisects the angle formed by these two planes will be the plane of polarization of one of the rays, that of the other being perpendicular to this plane, and passing through the given direction.

It follows from this, that if the light comes in a direction perpendicular to one of the axes of crystallization, one of the rays ought to have its plane of polarization perpendicular to this axis. The velocity with which these vibrations are propagated, depending only on the elasticity in the direction of this axis, it is evident that it remains the same whatever be the direction of the ray in the plane perpendicular to the axis. The other ray, on the contrary, whose plane of polarization passes through the axes, and consequently changes with its direction, will have different velocities in different directions, because its vibrations being always made in the plane of the other two axes of crystallization, may become successively pa-

rallel to both of these axes, and consequently undergo every change of velocity of propagation which the difference of elasticity in these two directions admits.

If therefore we cut a prism in such a manner that its edge is parallel to one of the axes of crystallization, that of the two rays whose plane of polarization is perpendicular to the axis ought to have a constant velocity, and follow in its refraction the law of Descartes (Snellius). The velocity of the other ray depends on its direction in reference to the other two axes of crystallization. Having thus cut three prisms, each of which had its edge respectively parallel to one of the axes of crystallization, and determining in each prism the index of refraction of the ray whose velocity remains invariable, we shall have *the three elements* on which the double refraction of the crystal depends.

The exposition of the results of the mathematical theory of Fresnel will illustrate still better what we have said. Calling, in the spirit of the system of emanation, v', v'' the velocities of the two rays, ε', ε'' the angles which the two optical axes form with the common direction of the rays, we shall have the velocity of one of these by the equation

$$v'^2 = A + B \cdot \sin^2 \tfrac{1}{2} (\varepsilon' - \varepsilon''),$$

and that of the other by the equation

$$v''^2 = A + B \cdot \sin \tfrac{1}{2} (\varepsilon' + \varepsilon''),$$

in which A and B are constants.

It has already been remarked, that two of the axes of crystallization are situated in the same plane as the optical axes, and that the third is perpendicular to this plane. I shall in the sequel call the axis of crystallization which bisects the acute angle of the optical axes the *axis A*, that which bisects the obtuse angle the *axis B*, and that which is perpendicular to the plane of the optical axes the *axis C*. From the preceding observations we conclude,

1. *If the edge of the prism is parallel to the axis A*, and if the two rays are consequently refracted in a plane perpendicular to this axis, we shall always have, if the angles ε' and ε'' are reckoned from the axis A, $\varepsilon' + \varepsilon'' = 180°$, and therefore

$$v'^2 = A + B \cdot \cos^2 \varepsilon'', \text{ and } v''^2 = A + B.$$

This last velocity is constant, and is that of the ray whose plane of polarization is perpendicular to the axis A.

The velocity of the other ray depends on the value of the angle ε'', which may vary from 90° to 90° $- \frac{1}{2} \alpha$, calling α the acute angle of the optical axes. The value of the square of this velocity will thus vary

between A and $A + B \sin^2 \frac{1}{2} \alpha$.

2. *If the edge of the prism is parallel to the axis B*, we have always $\varepsilon' = \varepsilon''$, and consequently

$$v'^2 = A \text{ and } v''^2 = A + B \sin^2 \varepsilon''.$$

The velocity v' is in this prism constant, and belongs to the ray which is polarized in a plane perpendicular to the axis B.

The velocity of the other ray depends on the value of ε'' between the limits $\frac{1}{2}\alpha$ and 90°. Hence the square of the velocity may vary

$$\text{between } A + B \text{ and } A + B \sin^2 \tfrac{1}{2}\alpha.$$

3. *If the edge of the prism is parallel to the axis C*, we shall always have $\varepsilon' = \varepsilon'' + \alpha$; hence

$$v'^2 = A + B \,.\, \sin^2 \tfrac{1}{2}\alpha, \text{ and}$$
$$v''^2 = A + B \,.\, \sin^2 (\varepsilon'' + \tfrac{1}{2}\alpha).$$

In this prism the velocity v' is constant, and belongs to the ray whose plane of polarization is perpendicular to the axis C.

As the angle ε'' may have different values from $90° - \frac{1}{2}\alpha$ to $-\frac{1}{2}\alpha$, the square of the velocity of the other ray will vary

$$\text{between } A \text{ and } A + B.$$

If in the three prisms, cut as now described, we observe the deviation of the ray, whose velocity remains constant independently of the direction, and if we calculate the index of refraction, we shall have the values of three quantities A, B and z. Calling n' the index in the prism whose edge is parallel to A, n'' that in the prism whose edge is parallel to B, and n''' that in the prism whose edge is parallel to C, we shall have, the velocity of light in air being taken as unity,

$$n'^2 = A + B, \; n''^2 = A, \; n'''^2 = A + B \,.\, \sin^2 \tfrac{1}{2}\alpha,$$

and consequently

$$A = n''^2, \; B = n'^2 - n'''^2, \text{ and } \sin^2 \tfrac{1}{2}\alpha = \frac{n''^2 - n'''^2}{n'^2 - n'''^2}.$$

I come now to an account of my experiments.

Arragonite.—Out of a crystal of this mineral from Bohemia, I cut three prisms:

1. *The prism A* having the edges of its refracting angles parallel to the axis A of the pyramidal crystal. A, No. 1, and A, No. 2, are two different refracting angles.

2. *The prisms B* had their edges parallel to the axis B; the two are marked B, No. 1. and B, No. 2.

3. *The prisms C* had their edges parallel to the axis C; two of them thus cut are named C, No. 1. and C, No. 2. The light which moves with a constant velocity may be known by its passing through a plate of tourmaline, having its axis parallel to the edge of the prism.

The prism A, No. 1.—Refracting angle 66° 43′ 17″. Temp. + 19° cent. In the spectrum, where the deviations were the

greatest, the ray F was brought to a minimum of deviation, and in the other spectrum the ray H.

The prism A, No. 2.—Refracting angle 51° 48′ 31″. Temp. + 18° cent. The rays F in the two spectra were brought to a minimum of deviation.

The following are the indices of refraction for the spectrum, whose plane of polarization is perpendicular to A.

Fixed lines.	Prism A, No. 1.	Prism A, No. 2.	Diff.
H	1·54226	1·54225	= 0·00001
G	1·53880	1·53885	− 0·00005
F	1·53480	1·53478	+ 0·00002
E	1·53264	1·53265	− 0·00001
D	1·53015	1·53011	+ 0·00004
C	1·52818	1·52822	− 0·00004
B	1·52747	1·52751	− 0·00004

These differences are evidently errors of observation, and the invariability of the velocity of the ray polarized perpendicularly to the axis A is consequently well established. With respect to the other ray, its velocity cannot be constant according to theory; and this is proved by observation, as is shown by the two following measures in the spectrum whose plane of polarization passes through A.

	Prism A, No. 1.	Prism A, No. 2.	Diff.
H	1·70996	1·70590	0·00406
F	1·69502	1·69128	0·00374

Prism B, No. 1.—Refracting angle 36° 13′ 30″. Temp. + 18°. In the spectrum with the greatest deviations, the ray F was reduced to a minimum of deviation, and in the other the ray H. The same was the case in

Prism B, No. 2.—Refracting angle 40° 12′ 3″. Temp. + 18°. The following were the indices of refraction.

	Prism B, No. 1.	Prism B, No. 2.	Diff.
H	1·71019	1·71004	0·00015
G	1·70325	1·70311	0·00014
F	1·69520	1·69510	0·00010
E	1·69091	1·69078	0·00013
D	1·68595	1·68583	0·00012
C	1·68206	1·68200	0·00006
B	1·68066	1·68057	0·00009

The differences here, though greater than in prism A, owing to the difficulty of cutting a face exactly perpendicular to the plane of the optical axes, are yet sufficient to prove the invariability of the velocity of the ray polarized perpendicularly

to B. That the velocity of the rays in the other spectrum varies, is proved by the two following observations.

	Prism B, No. 1.	Prism B, No. 2.	Diff.
H . .	1·54242 . .	1·54277 . .	0·00035
G . .	1·53493 . .	1·53529 . .	0·00036.

*Prism C, No.*1.—Refracting angle 29° 43′ 21″. Temp. + 17°. The ray H was in both spectra brought to a minimum deviation.

Prism C, No. 2.—Refracting angle 41° 34′ 32″. Temp. + 16°. In the spectrum of greatest deviation the ray F, and in the least the ray H, was brought to a minimum deviation.

The following were the refractive indices in the spectrum, whose plane of polarization was perpendicular to the axis C.

	Prism C, No. 1.	Prism C, No. 2.
H . .	1·70512	1·70505
G . .	1·69830	1·69843
F . .	1·69049	1·69058
E . .	1·68634	1·68635
D . .	1·68157	1·68156
C . .	1·67777	1·67781
B . .	1·67632	1·67630

The indices vary in the other spectrum, as the following observations show.

	Prism C, No. 1.	Prism C, No. 2.
H . .	1·55043	1·56158
F . .	1·54265	1·55331.

All these observations incontestably confirm the fundamental theorem of Fresnel, *that the velocity of one ray is invariable as long as its plane of polarization remains the same.*

The following means of the two systems of indices for the three spectra, whose planes of polarization are perpendicular to the three axes of crystallization, exhibit the elements of refraction of arragonite.

	Axis A.	Axis B.	Axis C.
H . .	1·54226 . .	1·71011 . .	1·70509
G . .	1·53882 . .	1·70318 . .	1·69836
F . .	1·53479 . .	1·69515 . .	1·69053
E . .	1·53264 . .	1·69084 . .	1·68634
D . .	1·53013 . .	1·68589 . .	1·68157
C . .	1·52820 . .	1·68203 . .	1·67779
B . .	1·52749 . .	1·68061 . .	1·67631

Calling n', n'' and n''' the indices for the spectra polarized perpendicularly to the axes A, B and C, and calculating the ratios $\frac{n'''}{n'}$ and $\frac{n'''}{n''}$, we shall find

	Ratio $\frac{n'''}{n'}$	Ratio $\frac{n'''}{n''}$
H . .	1·10883	1·00294
G . .	1·10681	1·00284
F . .	1·10449	1·00273
E . .	1·10322	1·00267
D . .	1·10154	1·00257
C . .	1·10066	1·00253
B . .	1·10024	1·00256

Hence every colour in arragonite has a double refraction as much greater as it is more refrangible. This result agrees with that for rock crystal and Iceland spar; and we may therefore conclude in general, that

Each colour has its individual double refraction as much greater as its own refrangibility is greater.*

By means of the preceding values of the indices n', n'' and n''', we may calculate the angle of inclination α of the optical axes by the formula $\sin^2 \frac{1}{2} \alpha = \frac{n'''^2 - n''^2}{n'''^2 - n'^2}$. as in the following table.

H . .	20°	25′	6″
G . .	20	12	6
F . .	20	0	50
E . .	19	53	0
D . .	19	37	8
C . .	19	33	14
B . .	19	44	40

Hence we see that in arragonite *the inclination of the optical axes diminishes continually from the violet to the red light.*

Dr. Brewster gives for the true inclination of the optical axes 18° 18′, calculated from the observed apparent inclination. But as he has not given the value of this apparent inclination, nor the index which he made use of to calculate the true inclination, it is impossible to compare his result with that of my experiments†.

Having several times measured the apparent inclination of the axes by means of a plate with parallel faces cut perpendicularly to the axis A, I found it a little more than 32°. To make a comparison with this value, we must calculate the *apparent inclinations from the true inclinations* as given in the above table. This is easily done; since we can now determine the velocity of light in the direction even of an optical axis. If we insert in the formulæ, given at the beginning of this section (p. 138), $\varepsilon'' = 0$, and $\varepsilon' = \alpha$, we obtain

$$v'^2 = v''^2 = n'''^2 - (n'''^2 - n'^2) \sin^2 \tfrac{1}{2} \alpha$$

in which $v' = v'' = n'$.

* See our last Number, p. 6. † See the next Article.

The ray which in emerging from the plate deviates according to the law of Descartes (Snellius), takes a direction without, making with the normal of the plate an angle $\frac{1}{2}\, i$, which is calculated by the formula $\sin \frac{1}{2}\, i = n'' \sin \frac{1}{2}\, \alpha$.

The following are the values of i for the different colours.

Apparent Inclination of the Optical Axes.

H . .	35°	10′	54″
G . .	34	39	30
F . .	34	10	0
E . .	33	51	10
D . .	33	17	46
C . .	33	6	24
B . .	33	24	22

The mean inclination is about 34°, which differs about 2° from the observed inclination. Notwithstanding the difficulty of taking this angle with precision, the difference of 2° is still too great. I cannot tell the cause, unless the two rays, which, within the plate, pass along the same optical axes, separate at their egress.

The preceding experiments having demonstrated that the ratio of the indices of refraction varies in the three spectra with the colours, the true ratio between the elasticities of the vibrating medium along the three axes of crystallization cannot be determined. If we take the elasticity of the vibrating medium in air to be unity, the elasticity along the axis A will be $= \frac{1}{n'^2}$, along $B = \frac{1}{n'''^2}$, and along $C = \frac{1}{n''^2}$; since the velocities being $\frac{1}{n'}$, $\frac{1}{n'''}$, and $\frac{1}{n''}$ in the system of undulations are as the square roots of the elasticity. But when the ratios $\frac{n'^2}{n'''^2}$ and $\frac{n'^2}{n''^2}$ change with the colours, they do not express exactly the ratios of the elasticity along the three axes of crystallization. However, in taking the elasticity along the axis A as unity, and calculating the above ratios for one of the middle rays of the spectrum, such as F, we shall have the following elasticities for Arragonite.

A	B	C
1	0·81975	0·82424

And for calcareous spar,

Along Axis.	Perpendicular to Axis.
1	0·79874

Colourless Topaz.—The prisms of this mineral were cut in the same manner as those of arragonite. As the two spectra always cover one another, I used a plate of tourmaline to

separate them in the manner already described for rock crystal.

Prism A, No. 1.—Refracting angle 30° 15′ 29″. Temp. +19°. In the spectrum of greatest deviation, the ray F was reduced to the minimum deviation, and in the other the ray H.

Prism A, No. 2.—Refracting angle 42° 40′ 16″.

The following were the indices observed in the spectrum perpendicular to A.

	Prism A, No. 1.	Prism A, No. 2.	Diff.
H	1·63506	1·63490	+0·00016
G	1·63123	1·63140	−0·00017
F	1·62652		
E	1·62408		
D	1·62109		
C	1·61880		
B	1·61791		

And in the spectrum polarized parallel to A.

	Prism A, No. 1.	Prism A, No. 2.	Diff.
H	1·62551	1·62758	0·00207
G	1·62156	1·62374	0·00010

Prism B.—Refracting angle 49° 3′ 8″. Temp. +19°. In both spectra the ray H was brought to the minimum deviation.

Prism C.—Refracting angle 38° 38′ 54″. Temp. 16°. In both spectra the ray H was brought to a minimum deviation.

The following are the indices for the spectrum polarized perpendicularly to the axes A, B and C.

	A.	B.	C.
H	1·63506	1·62539	1·62745
G	1·63123	1·62154	1·62365
F	1·62652	1·61701	1·61914
E	1·62408	1·61452	1·61668
D	1·62109	1·61161	1·61375
C	1·61880	1·60935	1·61144
B	1·61791	1·60840	1·61049

And the ratios $\frac{n'}{n''}$, $\frac{n'}{n'''}$ will be as follows:

	Ratio $\frac{n'}{n''}$	Ratio $\frac{n'}{n'''}$
H	1·00466	1·00595
G	1·00467	1·00597
F	1·00456	1·00588
E	1·00458	1·00592
D	1·00455	1·00588
C	1·00459	1·00587
B	1·00461	1·00591

These ratios differ so little from each other, that one would be led to regard the differences as only errors of observation. They, however, appear to increase a little from the violet to the red, and consequently do not contradict the result obtained for Iceland spar, rock crystal and Arragonite.

The inclinations of the optical axes calculated by the formula $\sin^2 \frac{1}{2} \alpha = \frac{n''^2 - n'''^2}{n'^2 - n'''^2}$ are as follows:

Inclination of the Optical Axes.

H . .	54° 54′ 0″
G . .	55 34 24
F . .	56 37 24
E . .	56 40 30
D . .	56 37 30
C . .	56 3 0
B . .	55 51 58

Abstracting the irregularities in these values towards the red extremity of the spectrum, it appears that *the inclination of the optical axes goes on diminishing with the refrangibility of the rays*, whilst the contrary takes place in Arragonite.

With regard to the value of the inclination, Dr. Brewster has found it = 65°, and M. Biot = 64° 14′. This difference of more than 8°, appears to indicate errors in the determination of the indices, unless the inclination in different specimens of colourless topaz is different, as Dr. Brewster found it to be for different kinds of Brazil topaz. It is to be observed, that all the prisms with which I made the preceding observations, came from the same topaz. Having after this only thin plates, I could not, on account of the great extent of the elliptical rings, measure the inclination of the axes with precision.

Taking for topaz as for Arragonite the elasticity along the axis A as unity, the following will be the elasticities along the other axes.

A.	B.	C.
1	1·01186	1·00922

In his memoir on Double Refraction (*Mém. de l'Institut*, tom. vii.) Fresnel has given from his experiments on diffraction made with colourless topaz, the ratio between the least and greatest velocity. He found it 0·9938. My experiments make the mean result $\frac{n'''}{n'} = \frac{1}{1{\cdot}00591} = 0{\cdot}99412$, which exceeds the former by 0·0003. Assuming the ratio 0·9938, and the inclination of the optical axes = 65°, the ratio $\frac{n''}{n'}$ may be found by the equation,

$$\frac{n''^2}{n'^2} = \frac{n'''^2}{n'^2} + \left(1 - \frac{n'''^2}{n'^2}\right) \sin^2 \tfrac{1}{2} \alpha.$$

We thus obtain 0·99560. My experiments give $\frac{n''}{n'} = 0{\cdot}99542$, which is 0·00018 in defect. These differences, however, evidently arise, on the one hand, from the difficulty of determining these ratios by experiments on refraction with prisms differently cut, with a precision comparable to that which we obtain by means of experiments on diffraction; and on the other hand from the probable inaccuracy in the value of the inclination of the optical axes found by the observation of the coloured rings.

XXXII. *Observations on the preceding Memoir. By* Sir David Brewster, *K.H. LL.D. F.R.S. V.P.R.S. Ed.*

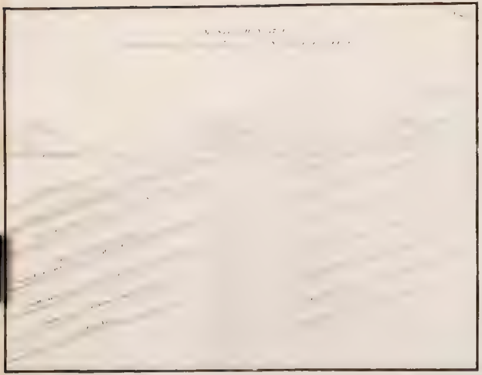
NOTWITHSTANDING the great accuracy and value of the preceding observations, and the importance of the deductions which the author has drawn from them, yet we are constrained to state, that almost all the general principles at which M. Rudberg has arrived have been previously established by English philosophers, though not by observations made by means of the fixed lines in the spectrum.

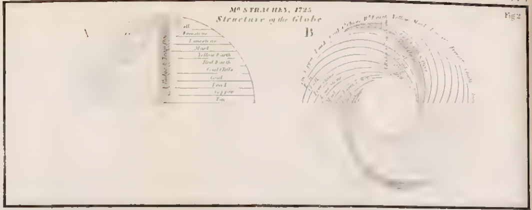
The variation of the inclination of the optic axes with the different colours of the spectrum, and the increase of that angle with the refrangibility of the colour in some crystals, such as Arragonite, and its decrease with the refrangibility in other crystals, such as topaz, is the discovery of Sir John F. W. Herschel, and one of the most important that has been made on the subject of double refraction; and yet the name of Sir John Herschel is not once mentioned. Sir John indeed did not examine *Arragonite* and *topaz*, but he found the very same phænomena in *sulphate of barytes* and *Rochelle salts;* and as I had myself discovered that all those crystals in which the inclination of the optic axes increases with the refrangibility, have the *red* ends of their systems of rings *inwards*, or towards the axis A; while those in which this inclination decreases with the refrangibility have the *red* ends of their rings outwards, or towards the axis B, and had determined that *Arragonite* had the red ends of its rings inwards, and *topaz* the red ends outwards*; the variation of the inclination of the optic axes in these two minerals, and its inverse character, were both known before M. Rudberg's experiments. To M. Rudberg, however, there remains the merit of having given the values of these angles, and that too in reference to fixed points of the spectrum.

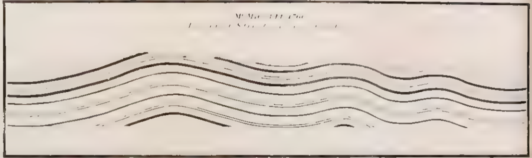
It is impossible to overlook the great difference between theory and observation in the inclination of the optic axes of Arragonite and topaz as given by M. Rudberg. His observa-

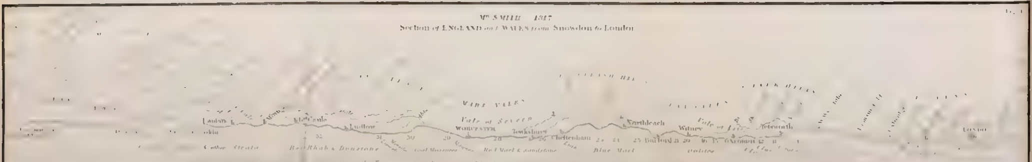
* Art. *Optics*, in the *Edinburgh Encyclopædia*, vol. xv. p. 596.

PROGRESS OF GEOLOGY IN ENGLAND









tions make the apparent inclination in Arragonite a little more than 32°, whereas the theory makes it fully 34°. According to my observations, the apparent inclination was 31° 15′, and the real inclination 18° 18′, computed with an index of 1·693. Upon re-examining this crystallized plate some years afterwards, I found that its surface was inclined to the axis A; and upon measuring the inclination, I found that the true inclination of the optic axes was 17° 33′, which corresponds to an apparent inclination of about 29° 56′ for the mean ray of the spectrum. Different crystals, however, have different inclinations; so that we are not entitled to compare this result with that which is deduced from theory.

The discrepancy between theory and observation is still greater in topaz, amounting to 8° in the inclination of the optic axes, if the specimen used by M. Rudberg had the same structure as that which was used by M. Biot and myself. Taking the index of topaz at 1·636, I found the apparent inclination of the axes to be 121° 16′, and the real inclination 64° 22′, which, from the excellence of the specimen which I used, cannot, I think, err above half a degree. M. Rudberg will no doubt measure the inclination of the axes in every specimen by which he has obtained the theoretical inclination.

In expressing a hope that M. Rudberg may continue his valuable observations, with other crystallized bodies, we trust he will excuse us for adding, that though the subject of double refraction is under the deepest obligation to M. Fresnel, yet other philosophers have wrought in the same field before him; and that his transcendent merits would not be diminished by a proper recognition and acknowledgement of their antecedent labours.

XXXIII. *Notes on the History of English Geology.* By William Henry Fitton, *M.D. F.R.S. &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

An article which I published in the Edinburgh Review for February 1818, On the Geological Map, and other works of Mr. William Smith*, having been frequently referred to, the historical part of it was printed, with some additions, in

† The following is a list of Mr. Smith's works, prefixed to the article in the Review,—vol. xxix. p. 310, &c.

"1. A Delineation (Map) of the Strata of England and Wales, with Part of Scotland; exhibiting the Collieries and Mines, the Marshes and Fenlands originally overflowed by the Sea, and the Varieties of Soil accord-

1821, for insertion in a Journal more immediately devoted to science than that in which it originally appeared; but its publication was at that time prevented by accidental circumstances. As the Review has been more recently mentioned by the late President of the Geological Society, in announcing the award of the first Wollaston Medal to Mr. Smith*, I now beg leave to place at your disposal one of the printed copies above mentioned.

I remain, Gentlemen, &c. &c.

London, July 1832. WM. H. FITTON.

A Map may not, at first sight, appear to come within the scope of a literary publication; but the performance now before us, with the other works connected with it, has more than ordinary claims upon the attention of the public. It contains a great deal of information, of practical importance as well as speculative interest. It is the first work of the kind that has ever appeared in England; and it is the production, after the labour of more than twenty years, of a most ingenious man, who has been singularly deficient in the art of introducing himself to public notice.

Mr. Smith is by profession a civil engineer, and, we are informed, is particularly skilled in that department of his business which relates to draining, and the structure of canals. It appears, that in the course of the inquiries to which his occupations naturally led him, he had occasion, many years ago, to observe the regularity and steadiness of the order exhibited by the strata in the vicinity of Bath; and about the year 1793, he drew up a tabular view of the stratification of that district, which in fact contained the rudiments of all his subsequent discoveries, and was in itself a proof of great sagacity and application. In the course of different journeys afterwards made, he not only recognised, among the strata in the North of England, several of his old acquaintances at Bath, but was surprised to

ing to the Variations of the Substrata, illustrated by the most Descriptive Names.—15 sheets, coloured. Carey, London. August, 1815.

"2. A Memoir of the Map and Delineation, &c. pp. 51. 1815.

"3. Geological Section from London to Snowdon. 1817."

"4. A Series of County Maps, on a much larger scale than that of the General 'Delineation,' &c., coloured to correspond with it. 1817.

"5. Strata identified by Organized Fossils, containing Prints on coloured paper of the most characteristic Specimens in each Stratum. 4to. Published in Numbers. 1816.

"6. Stratigraphical System of Organized Fossils, with reference to the Specimens of the original Geological Collection in the British Museum, 4to. 1817."

* See Phil. Mag. and Annals, N.S. vol. ix. p. 275.—EDIT.

find them in the same company with which they are associated in that neighbourhood: and, after full investigation, he became at last convinced, that the series of beds was uniform throughout the whole of the south-eastern portion of the island; and that the edge of every stratum, with very few exceptions, might be traced uninterruptedly, from one shore to the other, in a direction from S.W. to N.E. These important observations, which were made, we have no doubt, without any acquaintance with previous publications on the subject, led very naturally to the project of a map, in which they might be embodied and combined, and gave birth to the valuable works at present under our consideration.

It has been unfortunate for the celebrity of Mr. Smith, that he did not communicate to the public, in a more early stage of his inquiries, some general account of the principles which he had developed, with an outline at least of the detail. If, for example, he had given to the Royal Society a list and brief description of the English strata, his claims would have been recorded in the most dignified and authentic form, and his further progress would, no doubt, have been assisted by all those who felt an interest in the subject. His wish, however, seems to have been to abstain from publication till his project was completed; and the accomplishment of this purpose was from time to time delayed, in part, by his necessary attention to the pursuits of his profession; by the great expense attending an undertaking of such magnitude; and by his anxiety to give his work that perfection which every discoverer is naturally ambitious of conferring on his publications. In the mean time, as his inquiries advanced, he did not hesitate to make known the facts and inferences which occurred to him, and to exhibit freely his maps, sections and specimens *, with the warmth and liberality, and we may add, with the want of prudence, which are frequently characteristic of men of talents. Not only the elements, but a great part of the detail, of the present performances, were thus, in fact, made public; and the knowledge so diffused has had a most important, though unobserved, effect upon the labours of all succeeding inquirers; who were, perhaps unconsciously, but not less really, indebted to the author for very essential assistance in their progress, long before the productions now before us had actually issued from the press.

In an early stage of his inquiry, Mr. Smith communicated his observations to the Rev. Joseph Townsend, the author of

[* Some of the documents here referred to, of very early date, have recently been presented to the Geological Society:—1832.]—See Phil. Mag. and Annals, N. S., vol. ix. p. 281.—Edit.

a well-known book of travels in Spain; and subsequently to Mr. Farey, who was at that time we believe his pupil; two gentlemen who must, in fact, be considered as the chief editors of his opinions. The title of the book in which Mr. Townsend has given an account of Mr. Smith's discoveries, "The Character of Moses established for Veracity as an Historian*," was certainly not calculated to attract the attention of geologists, and has apparently very little connexion with the stratification of England; but the ingenious author conceived the credibility of the Mosaic account of the creation, to derive important support from the existing appearances of the globe; and, for the purpose of illustrating those appearances, he has entered into a full description of the British strata. He professes however, very candidly, to have obtained his knowledge of the subject almost entirely from Mr. Smith; of whom, after stating that, with a view to the completion of his own work, he had lost no opportunity of conversing with foreign mineralogists of eminence, he thus expresses his opinion:—

'The discoveries of this skilful engineer have been of vast 'importance to geology, and will be of infinite value to this 'nation. To a strong understanding, a retentive memory, in'defatigable ardour, and more than common sagacity, this ex'traordinary man unites a perfect contempt for money, when 'compared with science. Had he kept his discoveries to him'self, he might have accumulated wealth; but, with unparal'leled disinterestedness of mind, he scorned concealment, and 'made known his discoveries to every one who wished for in'formation. It is now (1813) eleven years since he conducted 'the author in his examination of the strata which are laid 'bare in the immediate vicinity of Bath; and subsequent ex'cursions in the stratified and calcareous portion of our island 'have confirmed the information thus obtained †.'

Mr. Farey, the other person above mentioned, who is himself a geologist of no inconsiderable merit, has not confined himself to the diffusion of Mr. Smith's opinions; but has very strenuously asserted the claims of his preceptor, not merely to having actually traced and demonstrated the order of the strata in England, and devised for their discrimination a number of subordinate distinctions, to which we believe his title cannot be disputed; but to his having been *the first* to ascertain 'that the fossil productions of the strata are not acci'dentally distributed therein, but that each particular species 'has its proper and invariable place in some particular stratum; 'and to having proved that some one or two, or more, of these

* Two vols. 4to. 1813-1815, Bath and London (Longman).

† Townsend, vol. i. Introduction, pp. 4, 5.

'species of fossil shells may serve as *new* and more distinctive 'marks of the identity of most of the strata of England *.' Now, upon these points we shall observe—1st, That we do believe Mr. Smith to have been led by his own observations to the discovery of the doctrines and facts which are claimed for him by Mr. Farey. But, 2ndly, It is equally certain, that a very near approach had been made by preceding writers to the doctrines maintained by Mr. Smith upon the subject of stratification; and, more especially, as to the possibility of deducing distinctive characters of the strata from their organized contents:—though it is only candid to allow, that the passages which bear upon these points might possibly have slept much longer in the volumes which contain them, if the attention excited by Mr. Smith's publications had not led to their detection; and that the light in which they now appear to us is very different from what it would have been without such assistance. 3rdly, That Mr. Smith deserves, undoubtedly, the credit of having first conceived, and actually executed, with extraordinary devotion, the project of tracing the strata entirely across this island; and of having thus established upon positive evidence, principles till then (at the utmost) considered rather as probable than as true. It is therefore very far from our intention, in the subjoined sketch of the progress of opinion and discovery respecting the newer and more regularly stratified portion of the globe, to detract from the great merit of Mr. Smith's investigations; or to impeach, if we may be allowed the expression, his consciousness of discovery: our sole object being to found the history of this subject, upon what we think must be regarded as the only safe and tangible standard in the chronology of science,—the relative order of publication †.

The French *Encyclopédie Méthodique* contains, under the article Physical Geography, published in 1796, by the late M. Desmarest ‡, a full account of some of the principal publications upon that subject, to the middle of the last century; from whence may be obtained some valuable facts, diluted very plentifully with speculation about the primeval state of the globe. But, on the whole, these volumes have not much increased our respect for the geologists of the last two centuries; and we can select from the list of philosophers whom they enumerate, the names of a few only who have given anything substantial to the science of geology. It is only fair to

* Phil. Mag. vol. li. p. 173, &c.

† [In this and some other paragraphs, in which additions have been made to the original paper, the style of the Review has been preserved, to avoid the necessity of changing the form of the whole.]

‡ *Encycl. Méthod., Geogr. Physique*, tom. i.

add, that we are far from supposing Mr. Smith to have been acquainted with these writings.

The zeal with which the collection of organized fossils was pursued during the latter part of the seventeenth century was very remarkable; and perhaps there is not any thing more extraordinary in the history of geological opinions, than the doctrine maintained at that period by Ray, Lister, and other eminent naturalists, respecting the substances now universally considered as the remains of animated beings. 'The great 'question now so much controverted in the world,' Dr. Plot tells us, in 1667, 'is, Whether the stones we find in the form 'of shell-fish, (and in his plates they are, with the caution usual 'at that period upon this subject, denominated 'formed stones,') 'be *lapides sui generis, naturally produced by some extraordinary 'plastic virtue, latent in the earth*, in quarries where they are 'found; or whether they rather owe their form and figure to 'the shells of the fishes they represent:*'—and this learned writer gives no fewer than seven reasons for adhering to the former of these opinions, in opposition to the sentiments of Hooke and other persons, who entertained more rational views. It will seem almost incredible to those who are acquainted with the works of Cuvier, and other inquirers of our days, that such a notion could at any time have found supporters: and it is the more strange that Lister should have maintained these views, as he was an excellent conchologist, and is to this day, we believe, considered as one of the best authorities in that department of natural history: yet Woodward says of him, that notwithstanding the strongest evidence, 'he *bravely continued 'to the last* firm and unshaken in his opinions †.'

This curious absurdity affords a good illustration of the danger of hypothesis in natural history; since it was connected with, if it did not originate in the assumption, that a general deluge was the *only* cause that could have occasioned the de-

* Natural History of Oxfordshire, p. 111.

† Catalogue, part ii. p. 6.—[The following specimen of Lister's reasoning upon this subject, will show, that notwithstanding his accordance with the great error of his day, he had some very just notions respecting fossil species. 'We will easily believe,' he says, '(what I have read in Steno's 'Prodromus) that all along the shores of the Mediterranean Sea, there 'may all manner of sea shells be found promiscuously imbedded in rocks 'or earth, and at good distance too from the sea. But for our English in-'land quarries, I am apt to think, there is no such matter as petrifying of 'shells in the business: but that these *cockle-like stones* are everywhere as 'they are at present, *Lapides sui generis*, and never were any part of an 'animal. It is most certain that our English quarry shells (to continue that 'abusive name) have no parts of a different texture from the rock in ques-'tion whence they were taken; that is, that there is no such thing as *shell* 'in these resemblances of shells, and that they never were any part of an

position of the bodies in question: For, as such an event must evidently have been too transitory to have produced appearances observable at great depths from the surface, and within the substance of strata in which no marks of disturbance were to be detected, there was no resource but in denying that the fossils of the solid beds had ever been endowed with life. The obstinacy with which the doctrine was adhered to, is no less surprising. Palissy indeed is praised by Fontenelle in 1720, for having overthrown it more than a century before *; yet in the year 1708, a book was published by Scheuchzer, under the title of *Piscium Querelæ et Vindiciæ*, where the fishes, entombed in stony substances, are represented as deploring, in very pathetic language, the indignity under which they suffer, in being degraded from the animal kingdom to the rank of mere inorganic matter. This remonstrance, however, does not seem to have been effectual; for Woodward, in 1723, still thought it necessary to reason against the doctrine we have mentioned: and afterwards, so late as 1752, M. Bertrand, a Swiss clergyman, made a last effort in its favour, contending that fossils are nothing more than links in the progressive series by which unorganized matter is connected with the animated world; or perhaps the unfinished materials ('*in fieri*,' as Dr. Plot had long before expressed it,) out of which the Creator might have formed, and in part did form, the existing races of similar beings.

In the Philosophical Transactions for 1684, there is published, '*An ingenious proposal for a new sort of maps of* '*countries*; together with tables of sands and clays, such as are 'chiefly found in the north parts of England, by the learned 'MARTIN LISTER, M.D.'†.—'We shall then,' the author be-

'animal. My reason is, that quarries of different stone yield us quite '*different sorts of species of shells*, not only from one another,—but I dare 'boldly say, *from any thing in nature besides, that either the land, or salt or* '*fresh water doth yield us.* 'Tis true that I have picked out of one quarry 'of Wansford very near resemblances of Murices, &c.; and yet *I am not con-* '*vinced that I did ever meet with any of these species of shells anywhere else* '*but in their respective quarries: whence I conclude them to be* Lapides sui 'generis, *and that they were not cast in any animal mould, whose species or* '*race is yet to be found in being at the present day!*'—Phil. Trans.—Lowthorp's Abridgement, vol. ii. p. 425.]

* *Encycl. Méthod.* tom. i. p. 406.— Bernard Palissy was born between 1514 and 1520. He delivered his opinions at Paris in 1575, in public lectures of which he has given an entertaining account in a treatise "*Des Pierres.*" His works were republished in 1777, by Faujas St. Fond; and Fontenelle is there quoted (among a crowd of authors who commend him) from *L'Histoire de l'Académie*, 1720, p. 5.

† Phil. Trans. vol. xiv. p. 739, &c. In the title, this paper is stated to have been 'Drawn up about 10 years since, and delivered to the Royal 'Society, March 12, 1683.'—As Dr. Lister lived till 1712, this precision as to dates seems to imply that his priority had been questioned.

gins, 'be the better able to judge of *the make of the earth*, 'and of many phænomena belonging thereto, when we shall 'have well and duly examined it, as far as human art can pos-'sibly reach, *beginning from the outside downwards*. As for 'the inward and central parts thereof, I think we shall never 'be able to refute Gilbert's opinion thereof, who will, not with-'out reason, have it altogether iron. And for this purpose, 'it were advisable that *a soile or mineral map*, as I may call it, 'were devised. The same map of England may, for want of a 'better, at present serve the turn. It might be distinguished 'into *countries*, with the rivers and some of the noted towns 'put in. The *soile might either be coloured, or otherwise distin-'guished by variety of lines or etchings; but the great care must 'be, very exactly to note upon the map, where such and such 'soiles are bounded.* As for example, in Yorkshire, 1. The '*Woolds;* chaulk, flint and pyrites, &c. 2. *Blackmoor;* moores, 'sandstone, &c. 3. *Holderness;* boggy, turf, clay, sand, &c. '4. *Western mountains;* moores, sandstone, coal, ironstone, 'lead-ore, sand, clay, &c. *Nottinghamshire;* mostly gravel peb-'bles, clay, sand-stone, Hall-playster or gypsum, &c. Now 'if it were noted how far this extended, and the limits of each 'soil appeared upon a map, *something more might be compre-'hended from the whole, and from every part, than I can pos-'sibly foresee, which would make such a labour well worth the 'pains. For I am of opinion, such upper soiles, if natural, 'infallibly produce such under minerals, and, for the most part, 'in such order.* But I leave this to the industry of future 'times.'

So far, therefore, as the *project* of a geological map, the credit of originality is clearly due to Dr. Lister; and this may be allowed to atone for his adherence to the absurd hypothesis already mentioned, as to the origin of fossil remains.

The arrangement of the "soiles" in Yorkshire, in the passage above quoted, accords with the more recent geological divisions of that county:—The *Woolds* apparently corresponding to the chalk formation; *Blackmoor* to the oolites, sands, and lias of the Eastern moorlands; *Holderness* to the deposits above the chalk; the *Western mountains* to the coal-formation with the subjacent limestones; and the gypsum, &c. of *Nottinghamshire*, perhaps to our red-marl and red-sandstone. There is nothing, however, relating to stratification in Lister's paper, nor to the order, or superposition, of the "soiles:" and the only point deserving of notice, in his 'scheme of sands 'and clays,' which is in general confused and erroneous, is, that in mentioning the sands of Boulogne and Calais, he observes that 'although that is not England, yet the sea has but ac-'cidentally divided us. For from Dunstable, *ex. gr.* in Eng-

'land, even as far as to the walls of Paris by Calais is, as it 'were, a continued *woolds* of chalk and flint.'

The geological labours of Woodward deserve very honourable mention; for he appears to have had some correct notions as to the general structure of the globe, and the proper method of pursuing the investigation of it; though his views were warped by the taste for antediluvian history which then prevailed, and his opinion that mineral substances were disposed in the earth according to the order of specific gravity, is singularly at variance with many of his own observations.

'I made strict inquiry (he tells us,) wherever I came, and 'laid out for intelligence of all places where the entrails of the 'earth were laid open, either by nature (if I may so say,) or 'by art and human industry. And wheresoever I had notice 'of any considerable natural spelunca or grotto, any sinking 'of wells, or digging for earth, gravel, chalk, coal, stone, mar- 'ble, ores of metals, or the like, I forthwith had recourse 'thereunto; where, taking a just account of every observable 'circumstance of the earth, stone, metal, or other matter, 'from the surface quite down to the bottom of the pit, I en- 'tered it carefully into a journal which I carried along with 'me for that purpose.—The result was, *that in time I was 'abundantly assured that the circumstances of these things in 'remoter countries, were much the same with those of ours here; 'that the stone and other terrestrial matter in France, Flanders, 'Holland, Spain, Italy, Germany, Denmark, Norway, and 'Sweden, was distinguished into strata or layers, as it is in 'England; that those strata were divided by parallel fissures; 'that there were inclosed in the stone, and all the other denser 'kinds of terrestrial matter, great numbers of shells, and other 'productions of the sea, in the same manner as in that of this 'island* *.'

The zeal with which Woodward devoted himself to natural history was very remarkable; and his *Catalogue of English Fossils* † is alone sufficient to entitle him to the gratitude of succeeding inquirers. The collection, to which the catalogue relates, is still preserved at Cambridge, and is to this day of great value as an object of reference:—and the professorship which bears his name in that University, in the hands of the able naturalists who have successively held the office, has

* Nat. Hist. of the Earth, 1723, pp. 4, 5.

† 'An Attempt towards a Natural History of the Fossils of England, &c. or a Catalogue of English Fossils' in the collection of J. Woodward, M.D. 2 tomes. Lond. 1728 and 1729.

contributed, and still continues powerfully, to diffuse a taste for geological inquiry.

A letter from the Rev. Mr. Holloway to Dr. Woodward, published in the Philosophical Transactions for 1723, gives a good description of the Fullers'-earth-pits near Woburn in Bedfordshire;—pointing out one of the most striking features in the physical geography of England; and connecting it so distinctly with the *order of the strata*, as to excite some surprise that the application of the principle was not sooner extended to other portions of the island.—'For the geographical situation of these pits, they are digged *in that ridge of sand-hills by Woburn;* which near Oxford is called Shotover; on which lies Newmarket-heath by Cambridge, and *which extends itself from east to west, everywhere, at about the distance of eight or ten miles from the Chiltern-hills,*—which in Cambridgeshire are called the Gog-Magogs, in Bucks and Oxon, the Chiltern-hills, from the chalky matter of which they chiefly consist: *which two ridges you always pass in going from London into the North, North-east, or North-west counties. After which you come into that vaste vale, which makes the great part of the midland counties*, and in which are the rivers Cam, Ouse, Nen, Avon, Isis, and others;—*which I take notice of, because it confirms what you say of the regular disposition of the earth into like strata, or layers of matter coming through vast tracts;* and from whence I make a question, whether Fullers'-earth may not probably be found in other parts of the same ridge of sand-hills among other like matter?'*

Stukeley, the celebrated antiquary, has pointed out the important fact in the disposition of the strata in England, that the steepest sides, or escarpments, are turned towards the west, or north-west: but he hastily generalizes this observation, and ascribes the fact gratuitously to the rotation of the globe†. The *Itinerary* of this writer contains many notices respecting the rocks and fossils of the districts he has described; to which his index refers, under the title of '*Memoirs towards a British Map of Soils*,' with allusion, apparently, to the project of Dr. Lister, already mentioned: and his notions about fossils appear to have been more correct than those of his predecessors.

* [Phil. Trans. vol. xxxii. p. 419.—Newmarket is here erroneously placed on the ridge of Woburn sands (now called the lower greensand): it is on the chalk, and the sands in its neighbourhood are above that stratum. The "question," at the close of the passage, has been justified by the discovery of Fullers'-earth in the lower part of the Woburn sands, almost throughout their course in England.]

† *Itinerarium Curiosum*, &c. By Wm. Stukeley, M.D. &c. London, folio, 1724, p. 3.

The opinion of Stukeley as to the effect of the rotation of the earth on the position of the strata, was not long after adopted by Mr. Strachey, the author of two valuable papers on part of the Somersetshire coal-district*, which, considering the date of their production, deserve particular attention. The first of these papers gives an account and section (Plate II. fig. 2.) of some coal-mines about ten miles S.W. of Bath;—detailing the order and composition of the beds,—and noticing their highly inclined position, their interruption by *ridges* (faults);—the occurrence above the coal-measures, of freestone (oolite), lias, and red-marl, in some places to the depth of 12 or 14 fathoms: but, it is added, while the coal beds of the country all dip about 22 inches in a fathom, '*the* (*superior*) '*beds of stone and marl, different from coal, lie all horizontal.*'

In the second of the papers above referred to, Mr. Strachey states, that as he had 'never heard any coal was found to the 'west or south of Mendip-hills; so Cotswold to the N.E., and 'the chalk hills of Marlbury Downs and Salisbury Plain, seem 'to set bounds to the coal country;' and in a section which accompanies this paper, he places the chalk horizontally above the lias, red marl, &c. and, like those strata, unconformably to the coal beds.

At the close of these descriptions the author extends his views, and having 'drawn,' as he tells us, 'the different 'strata (which have come to my observation) on a supposed 'plane, as they there lie; I protract the same in a globular 'projection, (see Plate I. fig. 2, A and B.) supposing the mass 'of the terraqueous globe to consist of the foregoing or per-'haps of ten thousand other different minerals, *all originally*, '*whilst in a soft or fluid state, tending towards the centre*; it 'must mechanically, and almost necessarily, follow, by the 'continual revolution of the crude mass from west to east, like 'the winding up of a jack, or rolling up the leaves of a paper 'book, that every one of these strata (though they each reach 'the centre,) must in some place or other, appear to the day, 'in which case there needs no specific gravitation to cause the 'lightest to be uppermost, &c. for every one in its turn, in 'some place of the globe or other, will appear near the surface; 'and were it practicable to sink a pit to the centre of the earth, 'all the strata that are would be found in that pit, and accord-'ing to the poet, *ponderibus librata suis.*'

* Phil. Trans. 1719, vol. xxx. p. 968; 1725, xxxi. p. 395: published also in a separate tract, entitled '*Observations on the different Strata of Earths* '*and Minerals, more particularly such as are found in the Coal-mines of Great* '*Britain,*' by John Strachey, Esq. London, 1729, 4to, p. 16. Fig. 2 of the annexed Plate is copied from this tract, and differs a little from that in the Phil. Trans.

We have copied the Plate connected with the former of these papers of Mr. Strachey (fig. 1.), because it represents very correctly one of the most striking geological features of the South-west of England, the unconformable position of the superincumbent beds, from the red marl upwards, to that of the coal strata*. And it will be perceived that the order of the strata given in the first "globular projection," (fig. 2. A) as derived from actual observation, coincides with that which modern inquiry has brought to light:—

Strata mentioned by Strachey.	Modern Names.
'Chalk	chalk.
'Freestone	oolites.
'Limestone ... } 'Marl ...	lias.
'Yellow earth } 'Red earth ...	red marl.
'Coal cliffs ... } 'Coal ...	coal formation.
'Lead, copper, &c.'	metalliferous rocks.

The second diagram (Pl. II. fig. 2. B.) is also inserted here, as it affords a striking proof of the very low state of geological speculation at the period of Mr. Strachey's inquiries; since an author, whose productions were thought worthy of publication by the Royal Society, and who appears to have been an excellent observer, could venture to connect with them so very crude an hypothesis.

The eloquence of Buffon had great effect in attracting attention, not only to the splendid speculations which may be connected with geology, but to the importance of organized remains, and to the light which may be thrown by them upon the structure and history of the globe. But the most remarkable views entertained about this period appear to have been those of Rouelle; though it is, perhaps, impossible, at present, to judge of the precise value of his labours; for, like Werner, he delivered his doctrines principally in lectures. He anticipated, or was coincident with Lehman, in the distinction (previously intimated, we believe, by Steno and Targioni,) of the primary from the newer rocks, under the denominations of *l'ancienne* and *la nouvelle terre;* and found reason also to make a division between the older and more recent of the secondary depositions, distinguishing the former by the title of *Travaille intermédiaire;* a discrimination and a name coming

* [This however now appears to be rather the exception than the general rule of structure. In the North of England, and in the Isle of Arran, the superior beds are *conformable* to those of the coal formation.—See Proceedings of the Geol. Society, p. 41, and Geol. Trans., 2nd Series, vol. iii. p. 33.]

very near to the *Transition class* of Werner,—whom he likewise anticipated in noticing the comparative rarity and peculiar character of the fossils contained in these intermediate rocks*. The account given by Desmarest, who was Rouelle's pupil, of his observations on the newer portion of the globe, and on the nature of the operations by which fossil bodies were distributed, is especially deserving of notice:—

'En examinant la nouvelle terre, et en observant les différens 'corps marins qui se trouvent si fréquemment et si abondam-'ment dans les couches horizontales, Rouelle reconnut que ces 'corps n'étoient pas jettés au hazard ni dans l'état de confu-'sion que l'on avoit imaginé communement avant lui. *Il vit 'que ces coquilles n'étoient pas les mêmes dans toutes les contrées: 'que certains individus se rencontroient constamment ensemble, 'tandis que d'autres ne se trouvoient jamais dans les mêmes lits, 'dans les mêmes couches;* et ce qui après cette même considéra-'tion, est très-important, il vit *que ces collections de coquilles 'fossiles, à la surface de certaines parties de nos continens, 'étoient dans le même état d'arrangement et de distribution que 'dans le bassin de la mer; où certains animaux testacés affectent 'de vivre ensemble attachés aux mêmes parages, et d'y former ces 'espèces de sociétés ou familles, de même que certaines plantes qui 'croissent toujours ensemble à la surface de la terre.*—En effet, 'une inondation passagère telle que le déluge, auroit dû met-'tre le désordre et la confusion par-tout, si l'on eût chargé ses 'eaux de transporter les corps marins dans l'intérieur de nos 'continens. Puisqu'au lieu de cette confusion, on reconnoît *un 'ordre constant dans l'arrangement des coquilles, dont certains 'individus font bande à part,* et ne se confondent point avec 'd'autres qui ont aussi leur familles séparées, il faut recon-'noître dans ces arrangemens non-seulement le travail de la 'mer, mais encore que ce travail n'a point été dérangé, par 'les événemens qui ont mis á sec nos continens de la nou-'velle terre †.'

Rouelle distinguished, under the general name of *Amas*, the various assemblages of fossil shells in the earth, and gave de-

* He placed the coal formation in the *intermediate* series. See *Encycl. Méthod., Géogr. Phys.* tom. i. pp. 412, 413, 477, 815: and compare with Jameson's *Geognosy*, pp. 80, 81, 146.—Rouelle was born in 1703, and died in 1770. No dates are given in Desmarest's notice of him; nor does his *Eloge* (*Hist. de l'Académie*, 1770,) contain any account of his geological opinions. Bernard de Jussieu, the botanist, was his friend, and the companion of many of his geological excursions. Some curious particulars about his lectures are to be found in the Baron de Grimm's Correspondence.

† *Encycl. Méthod., Géogr. Phys.* tom. i. pp. 416, 417.

nominations, to some of those which had fallen under his own observation in France, derived from the predominant species. In order to judge of the approach which he thus made to more modern opinions, greater detail would be necessary than we are possessed of. If the *amas* meant beds, the coincidence would be complete; and even what we have quoted indicates a very near approach to the principles, of which the French naturalists have since made such admirable use in their examination of the country round Paris; and which have furnished Mr. Smith with the title of one of his publications, *Strata Identified by Organized Fossils* *.

In a treatise which LEHMAN published in 1756†, he claims for himself the credit of being the first to observe and describe correctly the structure of stratified countries: but he does not seem to have been acquainted with the papers of Mr. Strachey above referred to. He supposes that *coal*-beds are the lowest of the stratified substances; that various 'pierres feuillettes' occupy the middle portion,—and the beds that afford the saline springs (fontaines salantes), the uppermost of the strata; which arrangement, he asserts, is universal. He has detailed the order, composition, and thickness of the strata, which surround the nucleus of the Hartz mountains, and occur in detached portions in the north-east of Germany; pointing out the identity of certain beds, which are separated from each other by intervals of several miles, but without asserting that the corresponding strata are absolutely continuous. His treatise is interspersed also with very good remarks upon the nomenclature and general relations of strata, and on the important purposes in practical mining, which may be promoted by the study of them; and his observations, we have reason to believe, are regarded in Germany as having thrown considerable light on the geology of that country.

[To be continued.]

* [Desmarest's exposition of Rouelle's doctrines, in a subsequent volume of the work last referred to, (ii. p. 346, &c., article AMAS, &c.) which however was not published until 1803, contains some passages expressing yet more nearly, the most recent views of geologists, as to the diffusion of organized remains, and their relations to the strata in which they occur. But there is still no distinct enunciation of the principle,—*that strata may be traced, in detached and remote situations, by means of their fossils*, which Mr. Smith had been acting upon for more than thirteen years, at the time of this last publication. The words "*font bande à part,*" in the latter part of the passage above quoted, are ambiguous; but they seem rather to relate to *horizontal* extent, than to vertical superposition.]

† *Versuch einen geschichte von Floetz Gebürgen*, Berlin, 1756. Translated by Holback, with other productions of Lehman, under the title of *Traités de Physique*, &c. Paris, 1759, vol. iii.

XXXIV. *Account of an Experiment in which Chemical Decomposition has been effected by the induced Magneto-electric Current.* By P. M.; *preceded by a Letter from* Michael Faraday, *Esq. D.C.L. F.R.S. &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

On returning to town yesterday, I found the inclosed letter: it is anonymous, and I have no means of referring to its author. But as it describes an experiment, in which chemical decomposition is for the first time obtained by the induced magneto-electric current, I send it to you for publication, if you think it worthy.

I cannot, from the description, decide whether the effect is really chemical: it may or it may not be so. A careful distinction must at present be drawn between real chemical decomposition and the mere effects of a succession of electric sparks. I hope the author will describe the results in a more precise manner, and corroborate them by other chemical actions.

I presume the writer can have no objection to the publication of his letter; and for my own part, I would rather avoid being in exclusive possession of anonymous philosophical information, lest any mistakes should hereafter arise as to dates. But if you publish the letter, favour me by thanking the author for it. I am, Gentlemen, yours, &c.

Royal Institution, July 27th, 1832. M. Faraday.

To Michael Faraday, Esq.

Sir,

From having read in the Proceedings of the [Royal] Institution your interesting papers on magnetism*, I was tempted to try an experiment, which succeeded beyond my expectations, and which, if tried on a larger scale, I am in hopes would prove very interesting.

I thought that, in place of making use of one powerful magnet, there would be considerably more effect (like in the Voltaic pile) by having a number of smaller magnets, connected with one wire or helix; and also, instead of getting the spark at making or breaking contact, it would be still better to make the instantaneous reversal of the poles the cause. I have contrived to do this in a very simple way; and with a

* See Phil. Mag. and Annals, N.S. vol. xi. p. 300, 465.—Edit.

small battery of magnets I have actually decomposed water. You will therefore excuse me for making this communication to you in this manner.

A wheel and axle is connected to a frame, and turned by the handle; a number of magnets (there must not be an odd number) is inserted round this wheel, and firmly secured in their berths, the wheel having spaces cut out to receive them, as shown in fig. 2.; two of the magnets are shown in their place at fig. 1, *b b*; in the same figure are the lifters, which are secured firmly to a board fast to the frame, as will be shown immediately. In placing the magnets in the wheel, which you perceive are the horse-shoe ones, every second magnet is placed differently. If the magnet at No. 1 has the north pole next the edge of the wheel, and the south next the axis, No. 2 has the south at the circumference, and the north at the axis, and so alternately; the ends of the magnets project a little beyond the surface of the wheel. There are as many lifters as magnets, placed firm in a board, exactly to correspond with the wheel, but made firm to the frame, and in such a manner as to permit the wheel to turn readily, so that the magnets will pass close to them. When any one magnet is in contact with a lifter, all the others are the same. In passing the wire round these lifters, care must be taken to make the turns of the helix be reversed in every second lifter, so that the currents of electricity will be all in one direction, although the poles of the magnets are reversed; by connecting the two ends of the wire to guarded points, and inserting them in a small tube containing water, on turning the wheel the decomposition will take place rapidly.

Fig. 1.

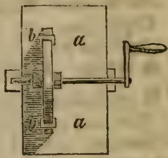
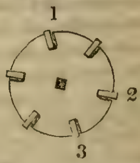


Fig. 2.



I put a small projector on the wheel at every magnet, which, on touching a spring, separated the two wires every time; and at the moment the pole was reversed, the spark became visible. Wishing you success in this very interesting field for discovery, I am, Sir,

Your very humble Servant,

P. M.

XXXIV. *Notices respecting New Books.*

The Microscopic Cabinet of select Animated Objects, with a Description of the Jewel and Doublet Microscope, Test Objects, &c.: to which are subjoined Memoirs on the Verification of Microscopic Phænomena, and an exact Method of appreciating the Quality of Microscopes and Engiscopes. By C. R. Goring, *M.D. Illustrated by Thirteen Coloured Plates from Original Drawings and numerous Engravings on Wood. By* Andrew Pritchard.

IN preceding Numbers of the Edinburgh Journal of Science, we have had occasion to speak very highly of the improvements in the microscope, which have been made by Dr. Goring and Mr. Pritchard, and of their joint work entitled *Microscopic Illustrations*, &c.

The work, of which we now propose to give a brief notice, though a separate and independent publication, may yet be regarded as a continuation of their former labours. The first thirteen chapters, which occupy about a third of the volume, contain popular and general descriptions of the aquatic larvæ of insects, crustacea and animalcula; and these descriptions are illustrated by *ten* beautiful and highly finished coloured engravings. The 14th and 15th chapters treat of jewelled microscopes and microscopic doublets. The 16th chapter, which is one of the most important and interesting of the whole (and of which we may give a separate account in a future Number), treats of the history of test objects, and of the method of viewing them. The objects here described are divided into two classes: 1. those which serve for exhibiting the penetrating power of microscopes; and 2. those which serve to exhibit their defining power. The *first* of these classes contain scales from the Lepisma saccharina, Morpho Menelaus, Alucita Pentadactylus, &c., Lycenæ, Clothes' moth, Pontia Brassica, Podura Plumbea, and Diamond Beetle. The *second* class contains Mouse hair, Bat's-hair, and Lycenæ Argus (the spotted feathers).

In chapter 17, Mr. Pritchard treats of microscopic doublets and other compound magnifiers for microscopes, and of their illumination. The microscopic doublet, as our readers well know, was introduced by Dr. Wollaston*. Mr. Pritchard has found that in such doublets the distance of the lenses which gives the best effect is equal to the *difference of the focal length of the two lenses, making a proper allowance for their thickness.* The proportion of the foci may be varied at pleasure: "All that is requisite in this respect," says Mr. Pritchard, "is, that the difference must be greater than the thickness of the anterior lens, while it may be observed (in high powers), that the greater the difference between their two focal lengths, the more space will be left in front; and as this is of great practical importance, they should never be less than as *one* to *three*. I have made some very good ones, differing as much as *one* to *six*. Another advantage resulting from attending to this point is, that we do not lose so much

* It is described in the Edinburgh Journal of Science, N.S. vol. i. p. 323. also in Phil. Mag. and Annals, N.S. vol. v. p. 300.

magnifying power in such combinations as when the difference between the lenses is less."

The 18th and 19th chapters, which are written by Dr. Goring, treat of the verification of microscopic phænomena, and of the exact method of appreciating the quality of microscopes and engiscopes.

These two chapters are of great practical value, and will be read with much interest both by the naturalist and the optician.

The last chapter of the volume is entitled *Miscellaneous Fragments*, and contains many useful directions and methods, especially in reference to the preparation and mounting of microscopic objects.

Such is a brief analysis of the work before us. We earnestly recommend it both to the general and the scientific reader as an original, a valuable, and an ingenious work; and we trust it will meet with such success as to disappoint the expectations of the author, who, in the following passage of his preface, has given a just though melancholy picture of the present state of our scientific literature.

"In the present forlorn state of scientific literature, it is rare that the author gets 'a return of his outlay,' and, indeed, very often loses one half, the demand for illustrated scientific books being less than for that of any other class: it is therefore in vain for an author to expect pecuniary remuneration for his time and labour. *All that is desired for this work is, that it may receive sufficient patronage to return its expenses.*"

XXXV. *Intelligence and Miscellaneous Articles.*

ON THE CAUSE OF THE PRODUCTION OF HEAT BY FRICTION AND PERCUSSION.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

YOU would much oblige me by explaining, in your valuable Journal, the "cause of heat from friction."

Iron laid upon an anvil and receiving violent blows from a hammer, will become red hot, partly from the diminished volume of the iron, and still more, as it is supposed, from an electrical cause. May it not arise, and chiefly, from the disengaged caloric latent in the particles of air which *must* intervene between the hammer or anvil, and the iron?

I am not aware of any experiments having been tried *in vacuo*.

Should you think this worthy of insertion in your Journal, you will oblige me. And I believe this idea has not occurred before to any one.

I am, Gentlemen, your obedient Servant,

E.

PREPARATION OF CHLORATE OF POTASH.

M. Liebig proposes the following process for obtaining chlorate of potash.

Heat chloride of lime till it ceases to destroy vegetable colours. In this case a mixture of chloride of calcium and chlorate of potash is obtained. This is to be dissolved in hot water, and to the solution, concentrated by evaporation, chloride of potassium is to be added, and then suffered to cool. After cooling, a quantity of crystals of chlorate of potash is obtained, which are to be redissolved and crystallized again to purify them. M. Liebig considers that this will be a cheap process for obtaining chlorate of potash. From 12 ounces of chloride of lime, of so bad quality that it left 65 per cent. of insoluble matter, he obtained an ounce of chlorate of potash.

The only difficulty to overcome in this process is, that the chloride of lime is not so easily decomposed by heat as is generally supposed; a solution of it may be kept boiling for an hour without losing its bleaching power. The best method is to form a thin paste with chloride of lime and water, and then to evaporate it to dryness: if it be required to prepare it by passing chlorine into cream of lime, it is advantageous to keep it very hot.

The chlorate of potash, which separates from the solution by crystallization, has not the form of scales, which it usually possesses, but is prismatic: whether this is occasioned by some admixture has not been ascertained; but on recrystallizing, it is obtained in the usual form.

The solution ought not only to be suffered to cool in order to procure crystals, for the crystallization is far from being terminated even after complete cooling; crystals continue to be deposited for three or four days.—*Ann. de Ch. et de Phys. tom.* xlix. *p.* 300.

COMPOSITION OF CAFFEIN.

MM. Pfaff and Liebig have given the following as the composition of Caffein:—

Four atoms carbon	3·05750	49·79
Five atoms hydrogen ..	0·31199	5·08
Two atoms azote	1·77036	28·83
One atom oxygen	1·00000	16·30
	6·13985	100·00

At the request of MM. Pfaff and Liebig, the analysis was also performed by M. Wöhler; and he obtained

Carbon	49·93
Hydrogen	5·43
Azote............	28·97
Oxygen	15·67
	100·00

Ibid. p. 305.

The following remarks, appended by MM. Pfaff and Liebig to the results of their analysis, appear to me to constitute a perfect specimen of the manufacture of mystery and confusion, which is likely, if anything can do so, to bring the atomic theory into discredit.

"According to its theoretical composition, caffein may be considered as a compound of a cyanic acid, containing one half less oxygen than the common acid, with æther analogous to cyanic æther. An æther formed of a problematical cyanous acid would be composed of $Cg^2 \frac{1}{2} O + (C^2 H^4 + OH^2) = C^4 H^5 N^2 O$; this formula is the same as that of caffein."

Now, among numerous other compounds, of which this compound may be supposed to be compounded, and for all that I see, with quite as much probability as those above stated, are

One atom of bicarburetted hydrogen,
One atom of ammonia,
One atom of cyanogen,
One atom of water. R. P.

EXPERIMENTS ON BEES' WAX AND VEGETABLE WAX.

M. Oppermann states that the vegetable wax from the East Indies is of a yellowish white colour, transparent at the edges, more brittle and greasy to the touch, but less compact than bees' wax. Its taste is rancid when it has been masticated for some time; its sp. gr. 0·97; at 124° Fahr. it melts, remains fluid at 112°, and solidifies at 109°.

It is soluble both in spirit and in æther; the former solution solidifies in cooling, while the latter merely deposits light flocks. Japan wax yielded by analysis,

Carbon	70·9683
Hydrogen	12·0728
Oxygen..........	16·9589
	100·0000

Brazilian wax very closely resembles the foregoing: their colour, consistence and odour are almost the same; the Brazilian is however distinguished by the yellowish brown pellicle with which it is covered; it fuses at 120°, and solidifies at 113°. The spirituous and ætherial solutions resemble those of the Japan wax. Brazilian wax gave by analysis,

Carbon	72·8788
Hydrogen	12·0297
Oxygen..........	15·0915
	100·0000

Bleached and purified bees' wax is harder than the foregoing; but the vegetable wax, dissolved in four parts of oil, gives a compound which is three times firmer than that obtained with the same quantities of bees' wax and oil; but the latter gives greater consistency to fat than the former.

Alcohol, even when hot, dissolves bees' wax with difficulty; the solution solidifies by cooling, and yields a white granular transparent mass. Æther when boiling forms a clear solution of bees' wax, which becomes turbid by spontaneous evaporation; it afterwards thickens, and the wax when separated appears to have suffered no change.

Caustic soda at first merely softens bees' wax, but afterwards converts it into soap, though not so readily as the vegetable wax.

By analysis, bees' wax yielded

Carbon	31·2910
Hydrogen	14·0726
Oxygen..........	4·6364
	100·0000

Ann. de Ch. et de Phys. xlix. p. 240.

LIST OF NEW PATENTS.

To J. J. Jaquier, Castle-street, Leicester-square, merchant, for improvements in the machinery for making paper. Communicated by a foreigner.—Dated the 31st of August 1831.—6 months allowed to enrol specification.

To H. G. Dyar, Panton-square, gentleman, for an improvement in tunneling, or method of executing subterraneous excavations. —5th of September.—6 months.

To G. Forrester, Vauxhall Foundry, Liverpool, civil engineer, for certain improvements in wheels for carriages and machinery, which improvements are applicable to other purposes.—5th of September.—6 months.

To W. Bickford, Tuckingwill, Cornwall, leather-seller, for his invention of an instrument for igniting gunpowder, when used in the operation of blasting rocks and in mining.—6th of September. —6 months.

To J. Neville, Great Dover-road, Surrey, engineer, for his improved apparatus for clarifying water and other fluids. —9th of September.—6 months.

To G. H. Palmer, Manchester-street, Gray's-Inn-road, civil engineer, for certain improvements in the steam-engines, boiler, and apparatus, or machinery connected therewith, applicable to propelling vessels, carriages, and other purposes.—16th of September. —6 months.

LUNAR OCCULTATIONS FOR AUGUST.

Occultations of Planets and fixed Stars by the Moon, in August 1832. Computed for Greenwich, by Thomas Henderson, *Esq.; and circulated by the Astronomical Society.*

1832.	Stars' Names.	Magnitude.	Ast. Soc. Cat. No.	Immersions.				Emersions.			
				Sidereal time.	Mean solar time.	Angle from		Sidereal time.	Mean solartime.	Angle from	
						North Pole.	Vertex.			North Pole.	Vertex.
				h m	h m	°	°	h m	h m	°	°
Aug. 4	49 Libræ...	5·6	1826	18 29	9 35	106	131	19 33	10 39	226	257

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Giddy at Penzance, and Mr. Veall at Boston.

Days of Month, 1832.	Barometer. London. Max.	London. Min.	Penzance. Max.	Penzance. Min.	Boston 8½ A.M.	Thermometer. London. Max.	London. Min.	Penzance. Max.	Penzance. Min.	Bost. 8½ A.M.	Wind. Lond.	Penz.	Bost.	Rain. Lond.	Penz.	Bost.
June 1	29·754	29·599	29·750	29·650	29·15	62	41	60	49	62	SW.	NW.	calm	0·20	...	...
2	29·859	29·846	29·850	29·850	29·37	70	49	64	45	56	NE.	SW.	calm	...	...	0·15
3	29·747	29·602	29·803	29·663	29·26	72	53	64	47	62	SE.	SE.	E.	...	...	...
4	29·537	29·514	29·466	29·440	29·10	63	52	63	52	57·5	SE.	SE.	calm	·14	...	...
☽ 5	29·552	29·488	29·416	29·410	29·00	65	46	62	50	57	S.	SE.	E.	...	0·280	...
6	29·521	29·463	29·460	29·416	29·00	66	46	62	47	60·5	S.	NW.	W.	·01	...	·97
7	29·697	29·573	29·513	29·466	29·05	61	50	63	48	61	SW.	SW.	W.	·53	...	·61
8	29·799	29·614	29·760	29·663	29·22	69	51	62	54	62·5	W.	NW.	NW.	·02	...	...
9	29·860	29·826	29·813	29·766	29·35	70	50	61	53	57	SW.	NW.	calm	·60	...	·09
10	29·894	29·863	29·819	29·660	29·40	73	52	65	49	56	SW.	SE.	calm	·01	...	·21
11	29·791	29·669	29·566	29·510	29·32	71	53	60	53	57	S.	SE.	calm	·22	1·400	...
12	29·566	29·468	29·819	29·660	29·08	71	56	65	54	64·5	S.	SE.	calm	·54	·110	·35
○ 13	29·573	29·514	29·360	29·360	28·93	74	54	63	55	68	S.	SW.	calm	...	·080	·02
14	29·825	29·649	29·760	29·566	29·04	70	54	62	53	64	SW.	W.	W.	·02	...	...
15	30·003	29·946	29·960	29·916	29·31	68	50	64	52	64	SW.	NW.	W.	·11	...	...
16	30·088	30·074	30·010	30·007	29·45	72	56	65	50	65	SW.	W.	NW.	·01	...	...
17	30·099	30·029	29·990	29·987	29·50	76	52	66	55	64·5	SW.	SW.	calm	...	...	·28
18	30·132	30·090	29·990	29·987	29·50	76	52	66	54	68	SW.	SW.	calm	...	...	...
19	30·117	30·044	29·980	29·977	29·43	77	52	66	55	70	W.	SW.	calm	...	·110	...
20	30·002	29·968	29·980	29·970	29·42	73	50	64	53	60·5	N.	NW.	calm	...	...	...
☾ 21	29·944	29·790	29·910	29·607	29·39	72	54	64	51	66	NE.	SW.	calm	·40	·540	...
22	29·699	29·516	29·760	29·610	29·05	67	51	64	52	57	SW.	W.	SE.	·04	...	·14
23	29·912	29·808	29·910	29·810	29·17	73	50	64	52	60	W.	NW.	W.	...	...	·26
24	30·005	29·946	30·060	30·060	29·28	65	50	63	52	61	W.	NW.	NW.	·02	...	...
25	30·085	30·011	30·107	30·063	29·40	68	47	63	52	57·5	W.	N.	NW.	·02	...	...
26	30·200	30·134	30·157	30·110	29·57	65	44	62	52	60	NW.	NE.	N.	...	...	...
27	30·330	30·281	30·224	30·210	29·71	73	55	66	52	63	N.	NE.	NW.	...	...	...
● 28	30·373	30·360	30·254	30·248	29·75	81	51	70	53	67	NW.	NE.	NW.	...	...	...
29	30·393	30·313	30·248	30·236	29·77	78	55	73	55	68	NE.	NE.	NW.	...	...	...
30	30·416	30·380	30·292	30·289	29·82	74	45	72	58	65	E.	SE.	NE.	...	...	...
	30·416	29·463	30·292	29·360	29·32	81	41	73	45	62				2·89	2·520	3·08

Remarks.

London.—June 1. Rain. 2. Clear and fine: hazy at night. 3. Overcast. 4. Cloudy: showers. 5. Overcast. 6. Showers. 7. Heavy showers, accompanied with thunder. 8. Rain: fine. 9. Rain, with thunder. 10, 11. Rain. 12. Sultry: heavy showers, with thunder in the afternoon. 13. Fine. 14. Showers. 15. Rain. 16, 17. Cloudy. 18, 19. Fine. 20. Sultry, with heavy clouds. 21. Hazy: slight showers in the afternoon. 22. Rain: clear. 23. Cloudy: fine. 24. Strong wind, with showers. 25. Fine, with heavy showers, occasionally. 26. Cloudy: clear at night, and rather cold. 27. Fine. 28—30. Fine, and very warm.

Penzance.—June 1. Fair: clear. 2—4. Fair. 5. Fair: rain. 6. Fair. 7. Fair: showers. 8, 9. Fair: misty. 10. Fair: rain. 11. Rain throughout. 12. Fair: rain: fair. 13. Fair: showers. 14. Fair: misty. 15—18. Fair 19. Rain: fair. 20. Fair. 21. Rain throughout. 22—24. Fair. 25—30. Clear.

Boston.—June 1. Fine: rain P.M. 2. Cloudy. 3. Fine. 4. Cloudy. 5, 6. Cloudy: heavy rain P.M. 7. Cloudy. 8. Fine: rain P.M. 9. Cloudy. 10, Cloudy: rain early A.M. 11. Rain. 12. Cloudy: rain P.M. 13—16. Fine. 17. Cloudy: rain early A.M. 18, 19. Fine. 20. Cloudy. 21. Fine. 22. Rain. 23. Cloudy. 24, 25. Stormy. 26. Cloudy. 27—30. Fine.

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[THIRD SERIES.]

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XXXVII. *On the Undulations excited in the Retina by the Action of Luminous Points and Lines.* By Sir David Brewster, *K.H. LL.D. F.R.S. V.P.R.S. Ed.**

IN the theory of vision, the light which radiates from visible objects is supposed to act only on those parts of the retina upon which it directly falls. To this principle, however, there are some exceptions. If a white circle is placed upon a green ground of some extent, the colour of the circle will not appear white but *red*, or the accidental colour of green. In like manner, if a narrow slip of white paper placed upon a green ground is viewed indirectly by the eye, it will vanish entirely as if it had been removed, and the space which it occupied will appear green. In both these cases the green light has acted upon a part of the retina upon which it did not fall, producing in the first case its accidental colour, and in the second case its own colour. When the retina is under the influence of a very strong light, the colour of every object which is painted upon it is either changed, or diminished in intensity, although the image is not formed upon any part of the retina which is directly affected by the strong light.

When light in the form of luminous lines in bright points acts upon the retina, a series of remarkable phænomena are produced, which, in so far as I know, have not hitherto been noticed. In giving an account of the experiments which I have made on this subject, I shall begin with the simplest case of a line of light.

* Read at the meeting of the British Association at Oxford, June 22, 1832.

1. If we look through a narrow aperture, about the 50th of an inch wide, at a bright part of the sky, or at the flame of a candle, we shall observe the luminous ground covered with a great number of broken parallel lines alternately light and dark. These lines are always parallel to the narrow slit, and of course change their place as the slit is moved round before the eye. If we look through a number of parallel slits such as the teeth of a comb, the broken parallel lines are seen more distinctly; and if we give the comb a motion oblique to the direction of its teeth, the broken lines become more distinct, though less straight than before, and new black lines appear lying in different directions as if they were detached portions of a number of dark ramifications. All these phænomena are seen more distinctly when we look at homogeneous light; but I have not been able to perceive any marked difference of magnitude in the spaces between the broken lines when they are formed by differently coloured rays.

If we use two systems of narrow slits and cross them at different angles, we shall perceive two systems of broken lines crossing each other at the same angles; and if when the lines of the two systems are parallel we give one of them a rapid alternating motion perpendicular to the direction of its slits, the parallel broken fringes are seen with peculiar distinctness.

2. Phænomena analogous to those now described, may be seen by looking at a number of parallel black lines drawn upon white paper, such as those which represent the sea in an engraved map, or by looking at the luminous intervals between a number of parallel wires seen against the sky. If the eye looks at any of these objects steadily and continuously, the black lines soon lose their straightness and their parallelism, and inclose luminous spaces somewhat like the links of a number of parallel chains. When this change takes place, the eye which sees it experiences a good deal of uneasiness, an effect which is communicated also to the eye which is shut. When this dazzling effect takes place, the luminous spaces between the broken lines become coloured, some with yellow, and others with green and blue light.

The phænomena produced in these two experiments are obviously owing to rectilineal undulations propagated across the retina; and the interference and crossing of the undulations, by which the dark lines are broken into detached pieces, and by which the colours are produced, arise from the unsteadiness of the head or the hand, which causes a want of parallelism in the successive undulations.

3. The action of small and bright points of light upon the

retina produces phænomena of a very interesting kind. If we look at the sun through a small aperture at a great distance from the eye; or, what is the same thing, if we look at the diminutive image of the sun formed by a convex lens or a concave mirror, or, seen in a convex surface, the light which falls upon the retina does not form a sharp and definite image of the luminous point, but it sends out in all directions an infinity of radiations covering in some cases almost the whole retina. These radiations are extremely bright, and are accompanied by mottled colours of great variety and beauty. The bright point of light propagates around it circular undulations, which are broken and coloured by interference, and which, being in constant motion from the centre of the retina in all directions, occasion the radiations which have been mentioned.

4. If we now look at the radiant image just described, through a narrow aperture, a very singular effect is produced. A vortex of circular rays appears on each side of the radiant point, and the rays have a rapid whirling motion. The line joining the centres of the two vortices is always perpendicular to the narrow aperture. This remarkable configuration of the rays is evidently produced by the union of a system of parallel undulations with a system of circular ones, the intersections of the parallel fringes and the diverging radiations forming the circular rays, as in the case of ordinary caustics.

The preceding phænomena, whatever be their true cause, clearly prove that light incident upon the retina exerts an action on parts of it upon which it does not directly fall, and that the same action renders other parts of the retina insensible to the light which actually falls upon these parts.

This remarkable effect is still more distinctly shown in the interesting experiment first described by Dr. Smith of Fochabers*. Upon looking with both eyes at a narrow slip of white paper, in such a manner as to see it double, he of course saw two slips equally white; but upon bringing a candle near one eye, the right one, for example, the image seen by the right eye became *greenish*, while that seen by the left eye was *reddish* white.

Dr. Smith remarks that the two colours are complementary, and form white light when the two images overlap each other. As the left eye was entirely protected from the light of the candle, and yet gave an image apparently complementary in its colour to that given by the right eye, it was difficult to resist the conclusion, that an influence was propagated from the *right* to the *left* eye through the medium of the optic

* Edinburgh Journal of Science, vol. v. p. 52.

nerves*. This conclusion, however, was the result of an imperfect examination of the phænomena; and I am persuaded, from a number of new experiments, that the following is the true explanation of the colours which characterize the two images.

When the light of the candle held close to the right eye acts upon one part of the retina, it renders every other part of the retina insensible, in a greater or less degree, to all other luminous impressions. The insensibility is a maximum close to the illuminated spot, and diminishes with the distance from it. Objects moderately illuminated actually disappear in the vicinity of the highly excited portion, and bodies of brilliant colours are not only shorn of their spendour, but have their tints entirely changed.

Dr. Smith observed that a *light red* slip of paper was seen of a *deep red* colour by the excited eye, and *nearly white* by the protected eye; while a *faint green slip* appeared a stronger green to the excited eye, and *almost white* to the protected one. If we use a stick of *red* sealing-wax it becomes of a *dark liver* colour to the excited eye, and of a *brilliant red* to the other eye. All bright blue colours have their intensity diminished in the excited eye; but those which are mixed with the less refrangible rays, or even with white light, become of a darker blue, that is, the depth of colour is increased, though the intensity of illumination is diminished. In the case of a compound *red* colour, such as that of red heat, the image seen by the excited eye is a decided yellow.

From these results it is obvious, that when the retina is excited by a strong light, the part of the membrane on which the light does not fall is rendered partially insensible to all colours, but in the greatest degree to red light. Hence it follows, that the white slip of paper should appear of a bluish-green colour, the complementary colour of red light. The red tinge which affects the slip of paper seen by the protected eye is the natural colour of candle-light heightened by the contrast of the green slip. As there is far less red in day-light than in candle-light, the slip seen by the protected eye is very much whiter in the former than in the latter. The sensibility of the illuminated part of the retina is affected in the very opposite manner. It becomes first insensible to blue light, a fact which is clearly proved by the experiments of Æpinus and others.

The influence of light on parts of the retina upon which it does not fall, is finely exhibited in an experiment which has

* See Art. *Accidental Colours*, Edinburgh Encyclopædia, vol. i.

never been explained. When a spectral impression of any very luminous body has become so weak that it can no longer be seen on a white ground, it is instantly revived by shutting the eye, and continues to be seen for a short time when the eye is again opened. The obliteration of the spectral image arises from the white light around it, extending its influence to the part of the retina occupied by the image; and the moment this action is removed by shutting out the light the original impression is revived, or rather is rendered visible by the removal of another impression which overpowered it.

Connected with these views is a very remarkable experiment described by Dr. Purkinje of Breslau, and which has been communicated to me by Mr. Potter, who has frequently and successfully repeated it.—If a candle is held a foot or two before one eye in a room without any other light, and is viewed directly by the observer, a mass of reddish-brown light is seen around the candle, and on this light, as a ground, are seen the ramifying blood-vessels of the retina, the base of the optic nerve and the *foramen centrale.* Mr. Potter finds that this experiment succeeds best when the candle is held about a foot from the eye, making an angle of about 20° with the line of distinct vision. I have tried it repeatedly, and under all forms, but I can see nothing excepting the mass of brown light. The prevailing explanation of this curious fact is, that the light which surrounds the candle is reflected back upon the retina, either by the inner concave surface of the crystalline lens, or of the cornea; and that the objects are, somehow or other, magnified by these concave surfaces. The moment I repeated this experiment, I recognised in the mass of nebulous reddish light the very same phænomenon which I had long before described as seen round luminous objects viewed indirectly. I have no doubt, therefore, that this light is propagated from the luminous image of the candle, and that though the retina, in contact with the blood-vessels, is sensible to direct light, it is insensible to propagated light, and therefore the blood-vessels must be delineated in obscure lines. As there is no retina across the *foramen centrale*, it will of course appear as a black spot; and owing to the obtuse vision of the optic nerve, it will appear less luminous than the surrounding retina.

In referring to the phænomena of indirect vision, I cannot avoid noticing the fact, that a candle seen by continued indirect vision appears more luminous than one seen directly. This led me to conceive the idea that it might be possible to generate, as it were, light by increasing its physiological action on the retina. Whenever we condense light for œco-

nomical purposes we merely change its direction, taking it away from one place and throwing it upon another; and in all such operations light is invariably lost: but if we can stimulate the retina and render it more sensible to a weak light by the mode of its application, we obtain the very same effect as if we had used a more powerful beam. The experiments which I have made on this subject have been more successful than I could have expected; and I hope to be able on some early occasion to submit them to the Association.

XXXVIII. *On an Instrument for Photometry by Comparison, and on some Applications of it to important Optical Phænomena.* *By* R. Potter, *Jun. Esq.**

WHEN engaged in examining the phænomena of the colours of thin plates in the form of what are generally denominated Newton's rings, I was surprised to find that the rings were so distinct in the transmitted light, and particularly when homogeneous light was used. These rings are now generally allowed to be produced, by the agency of the light which has been twice reflected at the surface of glass, under an incidence very nearly perpendicular.

Photometry had taught me that most of the common sorts of glass reflect about $\frac{1}{30}$th of the light incident upon them in this case; and we should expect two reflections to give an intensity of $\frac{1}{30}$th of $\frac{1}{30}$th, or $\frac{1}{900}$th of the first intensity. Now it requires very little consideration to see, that the presence or absence of so small a quantity of light is quite beyond detection by the eye, and experiments are easily executed by which it may be proved.

The difficulty of accounting for so great an effect being produced in a pencil of light, by so small a proportion of it, renders almost equally inadmissible every hypothesis which has yet been proposed to account for the whole phænomena of thin plates. On the doctrine of fits of easy reflection and transmission, which many experiments, otherwise, show must be dispensed with, even in the theory that light is caused by an emitted matter, this difference of the intensity of the dark and bright rings in the transmitted light is not to be satisfactorily accounted for. The theory that light consists of undulations in a subtile æther, gives scarcely any more admissible reason for the *whole* appearances, than the other. For in this theory the intensity of the light being taken as the amount of *vis viva* in the vibrating molecules, the modification which could be intro-

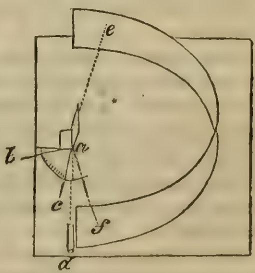
* Communicated by the Author.

duced by the interference of $\frac{1}{900}$th part of the whole *vis viva*, of the transmitted light, must be allowed, on all *mechanical* considerations, to fall exceedingly below so distinct an effect as that which we witness.

These considerations induced me to think upon the possibility of determining the relative intensities of the light in these dark and bright rings. This is not so difficult a problem as it at first sight appears to be; for though we may perhaps never expect to measure the light in the rings directly, yet it will be seen that if we can form variable appearances where we know the intensities of the lights, we may so vary them that they may form a representation of the phænomena we are considering; and then the only difficulty to be encountered is that of the eye judging with sufficient precision and accuracy, when the artificial is a correct representation of the natural effect. In experiments of this sort the only resource is that of repeated practice, by which the eye acquires a power of judging with an exactness far beyond what would be expected at the first trial.

There are evidently many ways of producing appearances where the intensities of the illuminations may be subjected to measurement and calculation. The instrument I have executed for the particular purpose above mentioned, is very manageable, but can only be used in the day-time, and in a particular state of the sky; that is, when it is either misty or uniformly overcast with clouds, so that a uniform illumination may be afforded to a piece of pasteboard, which is an essential part of the instrument.

The instrument I have used consists of a flat board of about 16 inches in length and 12 inches in breadth: on this board I fix edgewise a rectangular piece of pasteboard of about $23\frac{3}{4}$ inches in length and $3\frac{3}{8}$ inches in breadth. The edge of this pasteboard is fixed along a semicircle described on the board, as shown in the figure. In the centre of the circular arc is a pin, as at *a*, upon which turn the two arms *a b* and *a c*. Attached to each of these arms is a piece of crown-glass, which has been ground flat and polished; and afterwards covered at the further surface with black varnish to prevent reflection there. These pieces of glass are so fixed as when moving with the



arms round the pivot, to remain always perpendicular to the plane of the board; and hence, to an eye placed as at *d*, these glasses will reflect images of some portions of the surface of the pasteboard. If this surface is everywhere equally illuminated, the brightness of the reflection in the glasses will depend only on their inclination to the visual rays: thus when either glass reflects to the eye, the light from the part of the pasteboard almost opposite, or from *e*, the reflection will be very strong, and it will be weakest when the incidence is nearly perpendicular, or when the glass shows to the eye the part near *f*. Having a fixed position for the eye, or a tube through which to view the glasses, we easily determine the angle at which the light entering the eye is incident on the glass, by having a quadrant round the pivot graduated, and showing the inclination of the arms to the direction of the light.

It will now be evident that to produce a representation of the transmitted rings, we may view two narrow stripes of the glasses, covering any superfluous parts with blackened paper, and move the arms carrying them until the relative intensities of the reflections are sensibly the same as the relative intensities in the rings. Then knowing the angles of incidence upon the glasses, we can calculate the intensities of the light from a formula, which I have deduced from experiments, and published in the Edinburgh Journal of Science. The apparatus producing the rings with homogeneous light should also be attached to the board, or otherwise kept so conveniently that we may view them or their representatives alternately without any considerable space of time intervening; and the pasteboard should be coloured to the same tint as the homogeneous light made use of, to produce a more correct representation, and to prevent the eye from being deceived by any difference of colour.

The lights I have used in these experiments are a good homogeneous green, produced by a solution of arsenite of copper (Scheele's green) in muriatic acid, and a very perfectly homogeneous red, produced by a solution of iodine in hydriodic acid; this last solution gives most probably a purer colour than can be obtained of equal intensity by any other medium. I keep the solutions in small cut-glass phials with flat sides.

With the green light I found the rings produced by a lens of long focus* pressed upon a plane surface, to be represented

* I do not know exactly the curvature of this lens, but believe it to be to a radius of 15 or 16 feet.

in the photometer at the incidences given in the first and second columns of the following Table; the third and fourth give the quantities of light reflected; and the fifth the ratio of these quantities, the smaller one being taken as unity.

Glass for the dark Ring being set at Incidence:	Glass for the bright Ring requires to be Incidence:	Dark Ring contains:	Bright Ring contains:	Ratio, the dark Ring being taken as unity.
20°	63°	3·83	9·06	2·36
20	62	3·83	8·66	2·26
30	67	4·18	11·15	2·66
30	66	4·18	10·54	2·52
40	69	4·76	12·59	2·64
40	68	4·76	11·83	2·48
50	71	5·81	14·41	2·48
50	71	5·81	14·41	2·48

With the red light I obtained as in the next Table. The ratio of the intensities of the bright to the dark rings is here much greater than with the green light. I had expected it to be so in some degree, from the greater purity of the light, but not nearly to the extent which I found it. I repeated the first trials I made, very often, before I noted them down, and did not do so until I found clearly that the eye was not satisfied with any less difference.

Glass for the dark Ring being set at Incidence:	Glass for the bright Ring requires to be at Incidence:	Dark Ring contains:	Bright Ring contains:	Ratio, the dark Ring being taken as unity.
30°	70°	4·18	13·44	3·21
30	71	4·18	14·41	3·44
40	73	4·76	16·74	3·51
40	74	4·76	18·16	3·81

To calculate the intensity of the light in the third and fourth columns, I have used the formula which I have found for crown-glass in the essay above referred to; namely, of every 100 rays incident, those reflected are equal to $a + \frac{c^2}{r + b - x}$, where a, b, c and r, are constant quantities, and $r = 100$, $a = 2 \cdot 7$; $b = 1 \cdot 04$, and $c^2 = 76$; x being variable, and the sine of incidence to radius as 100.

The great difference in intensity between the dark and the bright rings which we here find, is certainly not to be accounted for on any principles of interference yet proposed; and it furnishes a very strong argument against the undulatory theory, in which the effects of interference are supposed to be perfectly determinate when we know the circumstances of the interfering pencils.

Dr. Young and Sir John Herschel have each given formulæ for this difference of intensity, which, it is important to know, nearly coincide; the slight difference between them arising only from the latter having introduced certain approximations to simplify the expressions he used.

Sir John Herschel has deduced his formula from the rules laid down by the late M. Fresnel; and he finds that the minimum of the light in the dark ring should be represented by the expression $1\cdot - 4a$, when the maximum of the light in the bright one is represented by $1\cdot$, and a is equal to the first reflection, and the light incident equal to unity.

Taking the value of $a = \frac{1}{30}$th, we have $1 - \frac{4}{30} = 1\cdot - \cdot 1\dot{3} = \cdot 8\dot{6}$

$$\text{and } \cdot 8\dot{6} : 1\cdot :: 1\cdot : 1\cdot 1538$$

or the intensity of the light in the bright rings should be to that in the dark ones, as 1·1538 to 1·, a result widely different from 3·5 to 1·, as we have found by experiment.

The great effect which we find to be produced by the interference of a small portion of light, must be deducible from any theory which is proposed as representing the true law in nature; and as a determined fact, it refutes an argument which M. Fresnel advanced against the hypothesis, that the fringes produced by the edges of bodies placed in a pencil of light diverging from a luminous point, are caused by the interference of light which has suffered an evanescent reflection with that which has arrived directly from the luminous point.

I have applied the photometer also, to repeating M. Arago's experiment, by which he has demonstrated that if the reflected and transmitted rings could be superposed they would exhibit a sensibly uniform light. I find this to be undoubtedly the fact; and the experiment furnishes an excellent means of trying the suitableness of the weather for using the photometer, and also the fitness of the locality where we purpose to experiment.

When I had completed the photometer, I found it very readily applicable to measuring the reflective powers of substances, of which we could never expect to procure sufficient extent of surface to render the method of photometry by lamps available: it requires only a very small extent of plane sur-

face to perform an accurate experiment with the comparative photometer.

For this purpose it is necessary to remove one of the pieces of crown-glass of the former experiment, and to place in lieu of it the substance to be examined, which has been before properly mounted, and then to find the incidence at which a *similar* surface on the piece of crown-glass gives an equal reflection. In this manner the larger facet of the diamond in a ring gave me the following results. The results in the first Table I obtained before the instrument was well adapted to the purpose; those in the second, which were obtained afterwards, I consider to be more correct.

I.

Incidence on Diamond.	Corresponding Incidence on Crown-glass.	Diamond reflects of every 100 Rays incident.
10°	65° 0′	10·00
20	65 0	10·00
30	67 0	11·15
30	65 18	10·15
40	68 48	12·43
50	74 0	18·16
60	78 12	26·80
70	79 18	30·05

II.

Incidence on Diamond.	Corresponding Incidence on Crown-glass.	Diamond reflects of every 100 Rays incident.
-3°	63°48′	9·41
10	63 48	9·41
10	63 24	9·23
20	64 48	9·89
20	64 36	9·79
30	67 0	11·15
30	65 42	10·37
30	66 42	10·96
40	71 12	14·62
40	70 0	13·44
50	74 18	18·63
50	73 42	17·72
60	76 0	21·65
60	75 36	20·87
70	79 0	29·11
70	77 42	25·48

These results are important, as we may compare them with the formula which has the uniform approbation of those who adopt the undulatory theory of light. The unanimous conclusion of Dr. Young, M. Poisson, and M. Fresnel, who have each investigated the subject, was, that the intensity of the perpendicular reflection, according to that theory, should be equal to $\left(\frac{\mu'-\mu}{\mu'+\mu}\right)^2$; and knowing the value of μ', or the refractive index for diamond, we find that this reflection ought, if the undulatory theory were true, to be about the double of

what it is in fact. Taking $\mu' = 2{\cdot}5$, we find $\frac{2{\cdot}25}{12{\cdot}25}$, or 18·36 rays should be reflected of every 100 incident; whilst experiment shows it to be only somewhat more than 9·, and perhaps about 9·3.

I have applied the photometer to a few other substances, and the results of my observations are given in the following Table.

Substance examined.	Incidence.	Corresponding Incidence on Crown-glass.	Reflected of every 100 Rays incident.
Mica	5°	20° 0′	3·83
Do.	5	19 0	3·80
Do.	20	25 30	4·01
Do.	20	25 0	3·99
Do.	30	34 0	4·38
Do.	30	35 0	4·43
Do.(another piece)	5	23 0	3·92
Do.	5	19 0	3·80
Selenite	3	3 0	3·49
Do.	5	10 0	3·60
Do.	45	45 0	5·20
Do.(another piece)	5	5 0	3·52
Do.	20	20 0	3·83
Iceland spar . . .	5	19 0	3·80
Do.	5	18 0	3·78
Do.	5	18 0	3·78
Do.	5	17 0	3·75
Do.(another piece)	5	22 0	3·89
Do.	5	22 0	3·89
Rock crystal . . .	0	14 0	3·68
Do.	0	14 0	3·68
Do.	0	12 30	3·65
Do.	0	16 0	3·73
Do.(another piece)	5	12 0	3·64
Do.	10	14 0	3·68
Amethyst	5	20 0	3·83
Do.	5	22 0	3·89
Emerald	10	22 0	3·89
Do.	10	24 0	3·95
Do.	30	40 0	4·76
Do.	30	40 0	4·76

The natural surfaces, and as *recent* as possible for mica, selenite, and Iceland spar, were used in all the above, except-

ing in amethyst and emerald; and I have also less confidence in the measurements for these two substances than for the others. The mica, selenite, Iceland spar, and rock crystal, were covered with a varnish of black sealing-wax on their second surfaces, to prevent reflection. Where it is said that the incidence on rock crystal was perpendicular, it must be understood that it was only so nearly so, that the natural unevenness of the facet made it impossible to determine it.

The reflection by selenite is so exceedingly nearly the same as that of crown-glass, that I found it impossible to state with certainty whether it was higher or lower: in one observation, however, I made it higher.

With the other substances, and particularly with mica and Iceland spar, the difference is quite obvious at the first view.

XXXIX. *On the Establishment of some perfect System of Chemical Symbols; with Remarks on Professor* Whewell's *Paper on that Subject. By Mr.* R. Warrington*.

IN entering upon the consideration of the necessity of chemical symbols,—a necessity which becomes the more urgent from the rapid progress the science is continually making, and from the increasing number of new combinations which are daily brought before our notice, and the want of some system of symbols to facilitate our reasoning upon these and other combinations,—there are two great points to be kept in view; namely, brevity and clearness in the nature of the symbols themselves, and as perfect an approximation to mathematical consistency and algebraic formulæ as the nature of the subject will admit.

Professor Whewell, in a paper upon this subject published in the 1st volume of the Journal of the Royal Institution for May 1831, advocates the necessity of radically altering the symbolic system of Berzelius, on account of its total want of mathematical propriety, and fully demonstrates the advantages to be derived from the adoption of an arrangement founded on algebraic principles †.

The improprieties more particularly pointed out in Berzelius's system of notation are; first, the method adopted by him of connecting the elementary symbols together in representing compound bodies, as though, according to the notation made use of in algebraic reasoning, the constituents were multiplied by each other; whereas the combination is effected by

* Communicated by the Author.

† In the Phil. Mag. and Annals, N.S., vol. x. p. 104, appeared a paper on Chemical Symbols and Notation by Mr. Prideaux, in reply to Professor Whewell, a brief rejoinder from whom will be found in the same volume. p. 405, note.—Edit.

the simple union or addition of the elements. As for instance, in Sulphuret of Potassium, one proportion of sulphur is added and chemically united to one proportion of potassium (*kalium*), which should be indicated by $S+K$, Sulphur + Kalium; but according to Berzelius's arrangement it would be written SK, in which the components are apparently multiplied by each other; and this, to use Professor Whewell's words (p. 441), " violates all mathematical propriety so entirely, that it must always be disagreeable to see an example of it for any person who has acquired the first rudiments of algebra."

The next point of consequence commented on, is the manner of representing compounds which contain more than one proportion of an elementary or compound body, and to which the prefixes, Bis, Tris, Dis, &c. have been given. The method pursued by Berzelius, is to place the numerals 2, 3, 4, &c. as indices over the symbol corresponding to the element, acid, or base: thus Bisulphuret of Iron, composed of two proportions of sulphur + one of iron, would be written $S^2\,Fe$; Bisilicate of Alumina, $\dot{S}^2\,\dot{A}$; Bisulphate of Copper, $\dddot{S}^2\,\dot{Cu}$; Disulphate of the Peroxide of Iron, $\dddot{S}\,\dddot{F}er^2$. To obviate these incongruities, and to lay before the chemical world a system formed on mathematical principles and consistent with algebraic formulæ, appears to have been the object intended by Professor Whewell in his paper: but in this he appears to me to have failed, not for want of due consideration and ability, but from the subject having been taken up in a mineralogical rather than a chemical point of view; for the Professor himself acknowledges, speaking of the proposed system (p. 448), that " the preceding notation is intended principally for the purposes of mineralogy;" and that " in the calculations of chemistry it would be necessary to have some additional contrivances. Thus it would be proper, as I have already observed, to indicate the mode in which both the oxides and the acids are formed from their bases by the addition of definite portions of oxygen." On attentively reviewing this part of the subject, I cannot help forming the conclusion, that these continual contrivances and contractions to suit different points of reasoning, must involve the subject in interminable confusion and difficulty. It would, I should consider, be far simpler to adopt one entire set of symbols applicable to all branches of chemical science, or to other sciences into which chemical reasoning may enter. If some arrangement of this kind is not fixed upon, the subject will be continually open to variation, and the caprice of different persons according to their several ideas of symbolic notation.

It is with the view of furthering the ultimate and, I hope, speedy establishment of some one systematic arrangement, that I have been induced to occupy a short space in your valuable Journal with the present paper, which, although the system promulgated in it should not be perfect, may yet be of service, as affording some useful hints to others more fitted for the final settlement of this most desirable and useful object.

On the first consideration of this subject, I was led to imagine that abbreviations of the English nomenclature would be more simple to the English student, and would be more readily understood and applied by him; but upon further reflection I was convinced that the Latin symbols, as selected by Berzelius, would be far preferable, on account of their having been in frequent use, more particularly among the Continental chemists and mineralogists, for some years, and also from their conciseness and simplicity. But (with one exception, which will be stated hereafter,) nothing more, I think, of Berzelius's system should be adopted, than the symbols of the elementary bodies. In the connection of these elements with each other, the methods generally adopted by Sir John F. W. Herschel, and subsequently followed by Professor Whewell in its principal features,—namely, the plus signs for the formation of compounds, and the use of brackets or ties,—must be taken to form the basis of a system with any claims to mathematical consistency:—an example will show more clearly the method according to which these are employed. Take, for instance, the octohedral copper pyrites, composed of two proportions of sulphuret of copper and one proportion of sulphuret of iron; this will be indicated thus, $2\,(S+Cu)+(S+Fe)$. Although this part of the subject has been cursorily alluded to in the former part of this paper, and the arguments used by Prof. Whewell briefly stated, yet I cannot avoid noticing in this place, that in a subsequent part of Professor Whewell's essay, he appears to have entirely forgotten the severe strictures that he had passed on Berzelius as to the want of algebraic consistency; and also his own observation (page 442), that "the combinations of ingredients which make up compounds are clearly of the nature of additions, and never can have any analogy with the multiplication of the numbers expressing the components; they therefore ought by no means to be represented by that combination of symbols which denotes multiplication." And again, at the 18th line of the same page, "there can be no doubt of the exceeding impropriety, I might say absurdity, of such a kind of symbols." Now, in direct contradiction to these observations, Professor Whewell proposes to represent the oxides of

the metals (p. 449) "by repeating the second letter of the symbol for each additional atom of oxygen, and attaching s (semissis) for the half atom: thus, Mn, Mns, Mnn, are the Protoxide, Deutoxide, and Peroxide of Manganese." Again, in objecting to the use of dots placed over the symbolic letters for the indication of the number of proportions of oxygen in any compound, it is admitted (p. 149) "that the notation is compact and simple," but "that it is not consistent with algebraic rule, as far as the oxygen is concerned;" and the writer argues, "that to be explicitly expressed, it should be done in the manner previously recommended, as fe + 2O, fe + 3O, Protoxide and Peroxide of Iron," according to Berzelius's view of those combinations. Now, on referring back to the preceding page, we find Professor Whewell urging the utility of representing the Acids commonly occurring in minerals, by an accent or dash placed over the bases of the acid: thus, for instance, S Sulphur, S′ Sulphuric Acid; C Carbon, C′ Carbonic Acid; Ar Arsenic, Ar′ Arsenic Acid; Cr Chromium, Cr′ Chromic Acid; Cl Chlorine, Cl′ Muriatic Acid; I Iodine, I′ Hydriodic Acid. And at the concluding part of the paper this system is extended to other combinations of oxygen with the same bases, the accent being varied: as S‵ Sulphurous Acid, C‵ Carbonic Oxide, Ar‵ Arsenious Acid. But in this arrangement no notice appears to have been taken of acids the basis of which combines with both oxygen and hydrogen, as is the case with chlorine, iodine, bromine, fluorine, sulphur, &c. The chloric acid must be indicated in the same way as the muriatic, the iodic as the hydriodic, sulphuretted hydrogen in combination as an acid the same as sulphuric acid, and so of the rest. Then we have also the perchloric acid, for which it would be necessary to form some other accent or distinguishing mark. Considering this subject impartially, it must be allowed, that Berzelius's arrangement, with respect to the representing oxygen by dots, if examined even in an algebraic point of view, is more simple and correct than the various accents made use of by Professor Whewell, each of which represents oxygen or hydrogen quite as fully as the dots indicate oxygen alone; and that no clew is afforded by this method as to the elementary bodies or their proportions which enter into the composition of a compound, but simply, that it is a union of an acid with a base, &c., and that it may be sometimes even doubtful what the nature of that acid is,—whether the acidifying principle be oxygen or hygrogen. Besides these contractions, there are others introduced equally objectionable; such as the representing the Metals by small letters, and their Oxides by the same letters, commencing with the large Roman character; as z′n Zinc, Zn

Oxide of Zinc; and also the indication of silica, alumina, and the other earths, by the symbolic letters of their individual bases, as S, A, &c.; and again with ammonia and water, they are respectively represented by the abbreviation Am, and the letter q. Professor Whewell states that these contrivances and contractions are to be considered as mere abbreviations, very convenient, but not indispensably necessary. If not so, why are they introduced? But the strict reason I imagine to be, that from Prof. Whewell's wishing to render the system perfectly mathematical, the different formulæ must necessarily be very extended, and therefore inconvenient. I consider these abbreviations uncalled for, however, and that they must add materially to the rendering any system of symbolic notation intricate and confused. The representation of oxygen by dots appears to simplify the subject, and render the formulæ very brief; and it clearly shows at the first glance the number of proportions of oxygen which enter into combinations, without at all confusing the arrangement, or rendering other contractions for the representation of the oxides or oxacids necessary, and it has therefore been adopted in the present system. I also propose introducing other dashes or marks to represent chlorine, iodine, bromine, fluorine, and nitrogen; for in the combinations of nitrogen with hydrogen and carbon, separately or conjointly,—as in ammonia, cyanogen, and their compounds,—this latter will be found of very great service. The way in which I should introduce these would be as follows: $H^{\cdot}$ one proportion of Hydrogen and one of Oxygen will represent water, the dot, as in the system of Berzelius, indicating the oxygen; $H^{|}$ Hydrochloric or Muriatic Acid, the vertical dash indicating Chlorine; H' Hydrobromic Acid, the acute dash being Bromine; $H^{\backprime}$ Hydriodic Acid, the grave dash for Iodine; $\overline{H}$ Hydrofluoric Acid, the horizontal stroke representing Fluorine; and $(3\,\underset{\cdot}{H})$. Ammonia, the dot beneath the symbols indicating Nitrogen; but in case this formula should not appear applicable, the one $\overline{3H+n}$ may be adopted. With respect to the half proportions, these may be readily and conveniently represented by making a fraction of the mark or accent thus: Fe, $\dot{Fe}$, $Fe^{\div}$ will be Iron, its Protoxide, and Peroxide. m., $m^{\cdot}$, $m^{\cdot\div}$, $m^{\cdot\cdot}$ Manganese, its Protoxide, Deutoxide, and Peroxide.

I fear that this introduction of accents and fractional accents will be condemned by those who wish to establish a system on pure algebraic reasoning. But I doubt very much whether a system can be so established without rendering the formulæ very extended, and in fact superseding the use of

symbols altogether, the beauty of which must always consist in their simplicity, clearness, and brevity. I shall annex some examples of the three methods; those of Berzelius, and Whewell, and the one here proposed; and leave the subject, I hope to be established speedily, by some one fully equal to the task.

O. Oxygen.	G. Glucinum.	W. Tungsten, Wolframium.
Cl. Chlorine.	Y. Yttrium.	
Br. Bromine.	Th. Thorium.	Cr. Cerium.
I. Iodine.	Zr. Zirconium.	Ni. Nickel.
F. Fluorine.	Al. Aluminum.	Co. Cobalt.
N. Nitrogen.	Si. Silicum.	V. Vanadium.
H. Hydrogen.	M. Manganese.	Cr. Chromium.
C. Carbon.	Fe. Iron, Ferrum.	Ta. Columbium,
S. Sulphur.	Zn. Zinc.	Tantalum.
P. Phosphorus.	Cd. Cadmium.	Hg. Mercury, Hy-
Se. Selenium.	Sn. Tin, Stannum.	drargyrum.
B. Boron. [sium.	Sb. Antimony, Sti-	Ag. Silver, Argen-
K. Kalium, Potas-	As. Arsenic. [bium.	tum.
Na. Natrium, So-	Bi. Bismuth.	Au. Gold, Aurum.
Li. Lithium. [dium.	Pb. Lead, Plumbum.	Pt. Platinum.
Ba. Barium.	Cu. Copper, Cuprum.	Pd. Palladium.
Sr. Strontium.	Te. Tellurium.	R. Rhodium.
Ca. Calcium.	Ti. Titanium.	Ir. Iridium.
Mg. Magnesium.	Mo. Molybdenum.	Os. Osmium.

	Chloride of Sodium.	*Nitrate of Potash.*
Berzelius.........	$Cl\ Na$	$\overset{\cdot\cdot\cdot\cdot\cdot}{N}\dot{K}$
Whewell.........	$cl + na$	$(n + 50) + (K + o)$
Proposed System	N'	$\overset{\cdot\cdot\cdot\cdot\cdot}{N} + \dot{K}$

	Nitrate of Ammonia.	*Muriate of Baryta.*
Berzelius..........	$NH^3\overset{\cdot\cdot\cdot\cdot\cdot}{N} + \dot{H}$	$Cl\ H\ \dot{Ba} + \dot{H}$
Whewell......	$(n + 3n) + (n + 50) + (h + o)$	$(cl + h) + (ba + o) + (o + h)$
Proposed System	$\overline{(3H).} + \overset{\cdot\cdot\cdot\cdot\cdot}{N} + \dot{H}$	$H' + \dot{Ba} + \dot{H}$

Or the same in another view,

	Muriate of Baryta.	*Alum.*
Berzelius...	$ClB + 2\dot{H}$	$\dot{K}\dddot{S} + \dot{al}^3\dddot{S}\,3 + 25\dot{H}$
Whewell	$(cl + b) + 2(h + o)$	$(\overline{K + o} + \overline{S + 30}) + 3(\overline{al + o} + \overline{S + 30}) + 25(o + h)$
Prop. Syst.	$Cl + B + 2\dot{H}$	$(\dot{K} + \dddot{S}) + 3(\dot{al} + \dddot{S}) + 25\dot{H}$
or,	$B' + 2\dot{H}$	

Hydrocyanate of Ammonia.

Berzelius............ NH^3Hc^2N

Whewell............ $(n+3H)+(\overline{2c+n}+h)$

Proposed System... $(3H).+\overline{(2c).+H}$

Persulphate of Iron and Potash.

Berzelius....... $2\dddot{S}\,1\tfrac{1}{2}\,\dddot{F}e+\dddot{S}\,\dot{K}+25\,\dot{H}$

Whewell...... $2(\overline{1\tfrac{1}{2}S+4\tfrac{1}{2}o}+\overline{fe+\tfrac{3}{2}o})+\overline{(S+30}+\overline{Ka+o})+25(h+o)$

Proposed Syst. $2(1\tfrac{1}{2}\dddot{S}+\dot{\bar{F}}e)+\dddot{S}+\dot{K}a)+25\dot{H}$

Ferrocyanate of Potash. *Cyanate of Lead.*

Berzelius... $2c^2\dot{N}K+c^2NFe+3\dot{H}$ $C^2No\,P\dot{b}$

Whewell $2(\overline{2c+n}+K)+(\overline{2c+n}+Fe)+3(h+o)$ $(\overline{2c+n}+o)+(pb+o)$

Prop. Syst. $2((2c).+K)+((2c).+Fe)+3\dot{H}$ $(\overline{2C).+o}+\dot{P}b$

Ammoniacal Alum.

Berzelius .... $3\,\dot{a}l\,\dddot{S}+NH^3\dddot{S}$

Whewell............ $3(\overline{al+o}+\overline{s+30})+(\overline{n+3h}+\overline{s+30})$

Proposed System... $3(\dot{a}l+\dddot{S})+((3H).+\dddot{S})$

30, Church-street, Spitalfields, July 1832.

XL. *Tabular Abstract of the Results of* Capt. Lloyd's *Leveling from the Sea near Sheerness to the River Thames at London Bridge. By* B. Bevans, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

CAPT. LLOYD's paper, as published in the Philosophical Transactions for 1831, which shows that the brass standard at the landing-place of New London Bridge is 2·3967 feet below an arbitrary mark at Sheerness, contains little information relative to the longitudinal section of the river Thames itself, either as to the surface of the water at high, mean, or low state; and which, in a philosophical point of view, would have been very interesting*. Most tidal rivers have the high-water mark at the outlet considerably *higher* than at a distance of some miles up the river, particularly when the country is flat, or almost level, through which the river passes; whereas it appears by these

* An abstract of Capt. Lloyd's paper, including a brief account of the apparatus and methods employed, was given in Phil. Mag. and Annals, N.S., vol. ix. p. 357.—Edit.

levels that the surface of the water at London Bridge, at *spring tide high water*, is nearly two feet above the surface at Sheerness. It may be worth inquiring why the river Thames differs in this respect from other rivers. It would not be very difficult to ascertain the nature of the curve formed by the surface of the water, both at spring and neap tides, by a simultaneous registering of the height of the water every half-hour for one day, at the respective stages of the moon and tides. There appear to be about eight or nine points where the course of levelling came in contact with the river, where Capt. Lloyd, doubtless, left plain and conspicuous marks to enable any person hereafter to refer to. One person at each of these points for two days, would enable us to see the variation of the surface of the water at the cardinal stages of the tide; and if such a development should be considered of sufficient interest, other periods of the tide might at an easy rate be added. At present, it is to be regretted that we know scarcely anything of the physical constitution of the most important river in the United Kingdom; while almost half the inferior rivers have had their longitudinal and transverse sections procured, showing the depth and capacity of the channel, and the power of discharge at different states of the tides.

The time will arrive when this knowledge will be important, and when an authentic record of the state of the river in times past will be of the greatest value.

The following is an abstract of the result of Capt. Lloyd's levelling, in reference to a natural zero of low water spring-tide.

8·5300	Mean level of the sea.
17·6150	Mean spring-tide high-water mark at Sheerness.
15·7600	Mean high-water mark.
13·9160	Neap tide high water.
15·3159	Marshes of Queenborough.
50·4265	St. James's standard.
19·0494	Boundary post of Hoo marshes.
15·7695	Marshes of St. Mary.
15·0455	Marshes of Higham.
24·1015	Three-mile stone on bank of Gravesend canal.
23·6849	Two-mile ditto.
22·8259	One-mile ditto.
22·2375	Iron clamp at second gate on Gravesend canal.
21·4983	Brass standard on pier at Gravesend.
18·7168	High water at Gravesend.
14·1922	Marshes at Northfleet.
16·9233	Boundary stone of Swanscomb.
12·8135	Marshes east of Dartford Creek.
11·9604	Marshes west of Ditto.

20·1142 Standard on Erith church.
11·5407 Marshes of Woolwich.
22·8830 Standard in arsenal, Woolwich.
23·2387 in corner stone near Officers' garden.
23·3993 □ top of $4\frac{1}{2}$-mile post, in river.
23·6786 □ on stone at west end of Dockyard.
23·3356 Brass standard in Dockyard, east point of mast slip.
31·6786 on small north-east gate-way from main road, Greenwich.
162·3618 Small brass standard under transit instrument, Greenwich College.
26·7968 Little brass standard on plinth of statue of Geo. II. Greenwich College.
23·5819 □ on iron plate near south side of lock on City canal.
19·7373 City canal $\frac{\text{HW}}{1800}$ Trinity.
23·0617 High tide, City canal, 21st of November 1827.
22·9829 Brass standard, West India Docks.
20·6462 XXIII. Ditto.
24·1229 Brass standard at Regent's canal.
21·8921 XXI. mark at Ditto.
20·1251 High-water mark on Ditto.
25·4381 Standard at London Docks, south-west pier.
19·6699 XXIII. 18 feet = $\frac{\text{HW}}{1800}$ Trinity.
18·0029 Mean high water, London Docks.
19·6511 Spring tide high water, Ditto.
1·6489 } Spring tide low water, Ditto.
1·6679 }
16·2739 Neap tide high water, Ditto.
10·5959 Mean level, Ditto.
37·7726 □ on top of granite post near entrance from Ratcliff Highway.
34·8457 on top of gun near eastern basin.
25·9954 Brass standard, St. Catherine's Dock.
19·7391 XXVIII. $\frac{\text{HW}}{1800}$ Trinity.
22·9665 Standard at Traitors' Arch, Tower.
19·5190 London Bridge, west side, $\frac{\text{HW}}{1800}$ Trinity.
19·2844 New London Bridge, brass standard at landing-place.

B. Bevan.

XLI. *Description of a Species of* Arachnida, *hitherto uncharacterized, belonging to the Family* Araneidæ. *By* John Blackwall, *F.L.S. &c.**

Tribe, Tubitelæ, } Latreille.
Genus, *Dysdera*, }

Dysdera Latreillii.

THE upper part of the cephalothorax is deep black, the under part being of a dark reddish-brown colour. The abdomen is almost cylindrical, very soft, hairy, and of a pale livid brown colour, each extremity inclining to yellowish-white above; the four exterior mammulæ or spinners are nearly equal in length. The legs, which are rather long, are marked with broad bands of brown and yellowish-white; the first pair is the longest, then the fourth, the third pair being the shortest; the two superior tarsal claws are deeply pectinated. The eyes are seated in the anterior part of the head; they are six in number, nearly equal in size, and form a small oval open in front, somewhat resembling a horse-shoe in figure. The mandibles are vertical; the maxillæ are long and dilated at the base externally, where the palpi are inserted: the lip is elongated, and gradually decreases in breadth from the base to the apex. In the male of this species, the sexual organs are situated near the termination of the palpi, on their under side, and are bent abruptly backwards; they are of an oval form, and have a delicate curved process near their extremity.

Length, from the anterior part of the head to the extremity of the abdomen, $\frac{1}{4}$th of an inch; length of the cephalothorax $\frac{1}{10}$th; greatest breadth of the cephalothorax about $\frac{1}{14}$th; length of an anterior leg $\frac{2}{5}$ths.

The above description is taken from a male spider; indeed, I have not yet been so fortunate as to procure a single specimen of the other sex. The first individual I met with was in a hedge at Oaklands, about two miles south from Llanrwst, on the Denbighshire side of the vale of Conway, North Wales. I afterwards found two other specimens in the crevices of a stone wall inclosing the ornamental grounds immediately adjoining Gwydir House, on the Caernarvonshire side of the same vale. The manners and œconomy of this species are at present unknown to me, but I hope soon to have an opportunity of investigating them.

I have carefully compared the new spider with a fine specimen of a male *Dysdera erythrina*, captured in Manchester by

* Communicated by the Author.

my brother, Mr. Thomas Blackwall; and perceive that there is not only a great disparity in size, and a wide dissimilarity in colour between the two species, (circumstances which might be supposed to arise from a difference in age merely,) but that they likewise differ very decidedly in figure and structure; thus clearly establishing the fact that they are specifically distinct. The former has the mandibles much less prominent, and the abdomen more nearly cylindrical than the latter; its tarsi also are destitute of brushes, with which instruments those of *Dysdera erythrina* are provided.

In adding another species to the solitary one at present constituting the genus *Dysdera* of M. Latreille, I avail myself of the opportunity to confer upon it the name of that illustrious naturalist, whose important researches have contributed so largely to the advancement of arachnology.

Crumpsall Hall, Aug. 10, 1832.

XLII. *An accurate Statement of Facts relative to a Stroke of Lightning, which happened on the* 13*th of April* 1832. *By* Benjamin Boddington, *Esq.**

On Friday, the 13th of April 1832, Mr. and Mrs. Thomas F. Boddington, having partaken of some refreshment at Tenbury, placed the servants inside their post-chariot, and mounted themselves the barouche seat behind, that they might enjoy the scenery on the road to Bromyard, through the ramifications of the Abberley Hills. It was about half-past three when they left, the sun shining, and the sky serene; but before they had proceeded far, they observed a dark and singular-looking cloud to arise, nearly in the direction of their route, and at the end of about three miles and a half a few drops of rain began to fall: they debated whether they should get inside the carriage, but agreed that the storm (for such it appeared to be) was passing off to the right, and that it would in all probability be only a slight shower, as the cloud in their immediate vicinity, though peculiarly dark and angry-looking, was of very small dimensions;—at this time a clap of distant thunder was heard, but no lightning seen. Mr. Boddington put up an umbrella; but perceiving that it was an old one, somewhat torn, (belonging to one of the servants,) he gave it to his wife to hold over her bonnet, while he put up another; when in the act of extending the latter, a flash of lightning struck them both senseless, threw the horses on the ground, and cast the post-boy to a considerable distance. The ser-

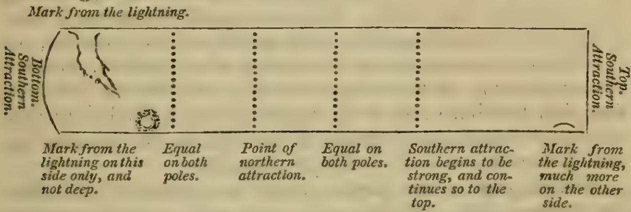
* Communicated by Mr. Faraday.

vants inside were untouched, and indeed unconscious of the real nature of the accident: the man says that he heard no previous thunder, but that a vivid flash of lightning, proceeding as he thought from the side of the road next to which he sat, was accompanied by an instantaneous report, like the discharge of a highly loaded blunderbuss; and he concluded that some robber, or other mischievous person, had shot the horses. He acknowledges that he was so panic-struck that for a few seconds he sat still; but on recovering from the momentary alarm, he let down the side glass and looked out to see whether his master and mistress were safe,—he was shocked to perceive the head of the former hanging over the seat, and apparently lifeless: he immediately jumped from the carriage, and ascending the steps behind, raised his master's head, and found that his clothes were on fire; his mistress was standing up, tearing off her bonnet and shawls. Her account of the matter is this: —that she neither saw the flash nor heard the thunder, but her first consciousness was the feeling of suffocation, and that she was pulling off her things to obtain air; she felt, however, that they had been struck by lightning, and immediately commenced assisting the servant to extinguish the fire that was still consuming the dress of her husband.

The passage of the electric fluid, as connected with Mrs. Boddington, was most distinctly to be traced: it struck the umbrella she had in her hand;—it was, as I before stated, an old one, made of cotton, and had lost the ferule that is usually placed at the end of the stick; so that there was no point to attract the spark: it was literally shivered to pieces, both the springs in the handle forced out, the wires that extended the whalebone broken, and the cotton covering rent into a thousand shreds. From the wires of the umbrella the fluid passed to the wire that was attached to the edge of her bonnet, the cotton-thread that was twisted round that wire marking the place of entrance over the left eye, by its being burnt off from that spot all round the right side, crossing the back of the head and down into the neck above the left shoulder; the hair that came in contact with it was also singed: it here made a hole through the handkerchief that was round her throat, and zigzagged along the skin of her neck to the steel busk of her stays, leaving a painful but not deep wound, and also affecting the hearing of the left ear. It entered the external surface of the busk:—this is clearly proved by the brown paper case in which it was inclosed being perforated on the outside, and the busk itself fused for about a quarter of an inch on the upper surface, presenting a blistered appearance. Its passage down the busk could not be traced in any way; there was no mark

whatever on the steel, nor was the paper that covered it discoloured or altered in the slightest degree; its exit at the bottom, however, was as clearly indicated as its entrance at the top; the steel was fused in the same manner, and the paper was perforated in the same way, but on the opposite side.

The magnetic properties acquired by the busk are curious. Both ends attract strongly the south pole of the needle, the upper part for some considerable way down; it then begins to lose power over the south pole, and the point of northern attraction is at about one third of the length of the busk from the bottom; so that by far the greatest portion of the whole has acquired southern attraction. Perhaps it will be best explained by the following sketch of the inside face of the steel, which is fourteen inches and a half long, by one inch and three-eighths wide.



There were marks of burning on the gown and petticoat above the steel; and the inside of the stays, and all the garments under the stays, were pierced by the passage of the fluid to her thighs, where it made wounds on both; but that on the left so deep, and so near the femoral artery, that the astonishment is, that she escaped with life;—even as it was, the hemorrhage was very great. Every article on which she sat was perforated to the cushion of the seat, the cloth of which was torn in a much more extensive way than the clothes: in most cases they were pierced by a hole not exceeding the size of half an inch in diameter, and even where the rents were larger they did not extend beyond an inch or two in any direction: but it is worthy of observation, that every article the electric fluid passed through had a singed appearance at the edges (and had a sulphureous smell, as I was informed by those who inspected them immediately after the accident: by the time I reached Tenbury, all trace of this smell had vanished). No ignition, however, took place beyond what occurred at the moment of its passage, notwithstanding the inflammable nature of most of the articles; nor did any of Mrs. Boddington's wounds present the appearance of burns. The cushion of the barouche seat was stuffed with curled horse-hair, through which the

stream must have passed, though no sign to indicate its passage was visible; the cloth edge of the cushion, however, immediately behind where Mrs. Boddington sat, was torn outwards, and the leather that covered the iron forced off in the same spot, clearly marking its egress at this place.

As this same iron also received the charge that struck Mr. Boddington, I shall now state the effects of the lightning on him, before I trace its further progress. When first discovered by his servant, he was, as I have said, insensible, and he remained in that state for about the space of ten minutes, when he revived sufficiently to inquire where he was, but relates that he was perfectly unconscious of what had occurred; that he felt his eyesight affected, and pain all over him, but knew not from what cause these sensations arose. The umbrella in this case also was the conductor; it was made of silk, and was but little damaged, a small portion of the upper part only being torn where it joins the stick, and none of the springs or wires being displaced. The main force of the shock, however, appears to have passed down the handle to his left arm, though a portion of it made a hole through the brim of his hat, and burnt off all the hair that was below it, together with the eyebrows and eyelashes; the fragments of the burnt parts falling into the eyes deprived him nearly of sight for two or three days, but the eyes were not otherwise injured. The electric stream shattered the left hand, fused the gold shirt-buttons, and tore the clothes in a most extraordinary manner, forcing parts of them together with the buttons to a considerable distance; and a deep wound was inflicted under its position on the wrist. The arm was laid bare to the elbow, which is presumed to have been at the moment very near his left waistcoat-pocket, in which there was a knife; this also was forced from its situation, and found on the ground; a severe wound was made on his body, and every article of dress torn away as if it had been done by gunpowder. From the knife it passed to the iron of the seat, wounding his back, and setting fire to his clothes in its passage. Another portion descended to the right arm, which had hold of the lower part of the stick of the umbrella; was attracted by the sleeve-button, where it made a wound, but slight as compared to that on the left, passed down the arm (which it merely discoloured, and broke the skin of in two small places,) to a gold pencil-case in the right waistcoat-pocket. The great-coat he had on was an old navy watch-coat, commonly called a *pea-jacket*, and of great thickness; this was torn to pieces, and the coat immediately above the waistcoat-pocket much rent; but the waistcoat itself was merely perforated; on the external part,

where the discharge entered by a hole about the size of a pea, and on the inside by a similar hole at the other extremity of the pencil-case, where it passed out, setting fire to his trowsers and drawers, and inflicting a deep wound round his back, the whole of which was literally flayed.

A very striking difference was observable in the wounds of Mr. and Mrs. Boddington: her's, as I before stated, were fractures of the flesh; his, on the contrary, whether deep or shallow, were all of them burns, and had a white and blistered appearance. The accumulation of force which the electricity acquired at this place deserves particular attention. I have observed that the shock on the right arm was nothing as compared to that on the left; the shirt-button was unchanged, and unmoved from its position, and the passage of the fluid down the arm barely indicated; yet when it arrived at the pencil-case, the amount of its intensity was such as to melt one end of it, and displace a cornelian seal at the other extremity, forcing it, I suppose, to some distance, as it has never since been found, though it was carefully sought after. It should seem that this accumulation of strength must have been derived either from the portion that passed over Mrs. Boddington, or from union with that which went down the left arm; in either case it appears to have been strangely diverted from its original course. The whole shock was now collected in the iron that formed the back of the barouche seat; the leather attached to it was torn off, and the iron itself broken in two, immediately opposite the spring, and the ends of the fractured parts bent forwards so as nearly to touch it: by this conveyance it is supposed to have diffused itself over the whole of the under carriage, and to have passed to the earth by the tires of the wheels, four holes being made in the road at the points they touched at the moment of the shock, though the carriage was not standing in them at the time it stopped. The post-chariot was a new one, and the only injury it received, was the fracture and derangement of the barouche seat, as already stated, the removal of the japan in a line along the bulge behind, and the breaking of the pole; the latter circumstance I conceive to have arisen, solely, from the fall of the horses, and to have been quite independent of the passage of the electric fluid. The horse the postilion rode was found to be dead; the other was evidently panic-struck, but unhurt, as he rose as soon as the harness was cleared from him; and though in a profuse sweat and trembling, he soon recovered, and not only was rode back for assistance, but returned again in the chaise that conveyed the poor sufferers to Tenbury, where they were detained at the inn for a month before it was thought safe to remove them.

On inspecting the dead horse no wound was visible, nor any apparent cause for his death; the brass front of the bridle was observed to be indented inwards, as if struck with a hammer; and when he was skinned, a corresponding mark was found on the bone of the head; and from that spot to the termination of the spine, the flesh was quite black and putrid for about the width of three inches, and there were diverging marks of the same nature on each side of the head, passing under the throat, and similar but much wider ones on the flanks. The post-boy was thrown some yards off, but this I conceive to have been by the spring of the horse when he was struck dead; and that spring doubtless jerked the carriage beyond the holes where the lightning had passed into the earth. The boy was shaken by his fall, but in other respects perfectly unhurt. I inspected the spot nearly three weeks after the accident happened, found it was elevated ground, but by no means the summit of the surrounding country; on the contrary, there were many higher hills in the neighbourhood: the road itself was so much hollowed out, that the banks must have been nearly equal to the height of the carriage; in a field to the right, within a few yards of the hedge, and exactly opposite to where the shock took place, was a very high pear-tree,—it however bore no trace of injury. The carriage appears to have been passing close to that side of the bank, as the holes I have before alluded to were still perfectly visible; indeed, the two to the right had undergone very little change, as they were nearly off the road; they were about fifteen inches in diameter, perfectly round, and nearly as deep as they were wide, the stones appearing to have been thrown out as if done by a miner's blast.

The collateral facts must now be mentioned. The landlord of the inn at Tenbury informed me that he was sitting in his parlour, talking to another person, when he saw the flash of lightning that must have caused the accident; he observed to his companion, that he had never before seen so singular a flash, as it appeared to divide into four parts when it came within about thirty yards of the earth;—this statement was confirmed by the person who was with him. It should seem, therefore, that they were not struck by a single discharge of electric matter, but were enveloped in a mass of electricity; and this is the more probable, from the traces of the different strokes being so distinct, and yet taking such opposite directions: the fluid seems to have pervaded the whole atmosphere, as many things were magnetized that were not in the line of any of the tracks that could be traced. For instance, Mr. Boddington's watch was in his fob, and quite out of the line described by either of the shocks that passed over him:

after the accident, it was found necessary to send it to a watchmaker, and when taken to pieces, parts of it were discovered to be highly magnetized, the balance-wheel in particular. This was shown to Mr. Faraday, when at Oxford, who set it afloat on a cork, and found the poles to be so well defined, that I have since had it mounted as a compass. Two pair of scissars also that were in Mrs. Boddington's work-box inside the carriage, were by mere accident, two months after the event, discovered to be magnetic.

I certainly now very much regret that more minute researches were not made at the time as to these facts: but whoever has watched over the sick-bed of a beloved son, with but faint hopes of his recovery, will not be surprised that philosophical investigations were all absorbed in the deeper interest of the affections.

Badger Hall, July 16, 1832.

XLIII. *Further Experiments with a new Register-Pyrometer for Measuring the Expansion of Solids.* *By* J. Frederick Daniell, *Esq. F.R.S. Professor of Chemistry in King's College, London.**

IN my former communication on a new Register-pyrometer, which has been honoured with a place in the Philosophical Transactions for 1830, I stated that I hoped, at some future period, to be able to lay before the Society the results of some experiments upon the dilatation of metals to their melting points; and I now purpose to redeem this pledge.

My previous observations upon the subject of expansion, were directed chiefly to the object of establishing what degree of confidence might be reposed in the instrument as a measure of temperature; and I was able, I trust, to exhibit such an accordance between the measures which it had afforded and those of the best experimenters, long previously obtained with various metals to the boiling point of water, as fully to establish its sufficient accuracy. The comparison however which I most relied upon, was with the experiments of MM. Dulong and Petit, upon the expansion of platinum and iron to the high temperature of 572° Fahr.; and as this is a point of fundamental importance, I shall still further strengthen it by a comparison with the results obtained by the same distin-

* From the Philosophical Transactions for 1831, Part ii.: this paper was read before the Royal Society, on the 16th of June in that year.

Prof. Daniell's former communication on the same subject will be found in the Phil. Mag. and Annals, vol. x. beginning at p. 191.

guished philosophers with copper, the only other solid metal to which they extended their inquiries.

Previously to this, I trust it may not be thought tedious, if I briefly relate the results of some trials for obtaining registers of uniform composition, which might preclude the necessity of determining the rate of expansion in each individual instance.

Exp. 23.—For this I had recourse to Wedgwood's ware, of which I obtained some bars carefully constructed and highly baked for the purpose. The expansion of these I found precisely equal to that of platinum; so that when the register was immersed in boiling mercury, the index was found not to have moved. When a bar of iron was substituted for that of platinum, the arc measured was 1° 7′.

With black-lead the same expansion gave a measure of 2° 49′, from which if we deduct the expansion of platinum in black-lead 1 45

the remainder 1 4

is sufficiently near to confirm the result.

Exp. 24.—My next trial was with registers of black-lead of various and known mixtures of plumbago and Stourbridge clay. Four-fifths proportion of the former to one-fifth of the latter produced a composition which was too tender for the purpose; but a mixture in the proportion of three-fourths to one-fourth formed a ware of a fine, even texture; whose expansion was very equal, and not exceeding the least of those which I had formerly tried.

Three different registers of this composition afforded me the following measures of the expansion of a platinum bar to the boiling point of mercury.

1° 45′ 1° 42′ 1° 38′.

To which I may add a fourth, which gave for the expansion of an iron bar to the same point an arc of 2° 42′, which is equivalent to 1° 40′ for a platinum bar. For all common purposes, therefore, the mean expansion of 1° 42′ might have been adopted without any serious error in the final results. In investigations, however, which require the utmost precision, I still think it advisable to fix the expansion of each register by experiment.

Exp. 25.—A bar of copper was adjusted in one of the registers and exposed, in the manner formerly described, to boiling mercury; the arc measured on the scale was 4° 10′, equivalent to an expansion of ·03633.

Let us now compare this result with the determination of MM. Dulong and Petit, as we formerly did the expansions of platinum and iron.

The expansion of Copper.

	Length of Bar.	
From 32° to 212° = ·0017182 × 6·5	.... =	·01116830
From 392° to 572° = ·0018832 × 6·5	.... =	·01224080
		·02340910
From 212° to 392° = Mean of the above......	=	·01170455
Total expansion from 32° to 572°	=	·03511365
Add for the expansion from 572° to 660°, the temperature of boiling mercury, calculated at the highest rate:— 180° : ·0018832 : : 88° : ·00920675	=	·00920675
		·04432040
Deduct expansion for 32°, the experiment with the pyrometer having commenced at 64°....	=	·00305457

Calculated at the lowest rate:—
180 : ·0017182 : : 32° : ·00305457

Real expansion of the bar by Dulong and Petit = ·04126583

If from the real expansion thus obtained	·04126
we deduct the apparent expansion obtained by the pyrometer...	·03633
The remainder	·00493

will be the expansion of the black-lead.

We thus obtain the expansion of 6·5 inches of black-lead ware,

from 64° to 660° by platinum bar	·00421
by iron bar	·00457
by copper bar........................	·00493
Mean	·00457

in which the extreme results differ from the mean not ·0004 inch, or one-fourteenth of the whole.

When we take into consideration the great difference in the total expansion of these three metals, as well as the differences in their several rates of increase with the increasing temperature, such an accordance appears to me to be perfectly decisive of the accuracy of the pyrometer.

It will be unnecessary for me to trouble the Society with the details of the experiments by which I determined the expansion of several other metals to the boiling point of mercury; it will be sufficient to state the results in a tabular form. I thought that it would add much to the interest of the de-

termination of the total expansion to the fusing points, to determine previously the expansion of each to the points of boiling water and boiling mercury; that any alteration in the rates of expansion between these points might be detected.

I must, however, make a few observations upon the general method which I adopted to insure an accurate determination of the former.

Exp. 26.—Judging from the action of the pyrometer at lower heats, I expected that the index would continue to be thrust forward by the progressive expansion of any bar of metal, till its cohesion gave way and it assumed the fluid form; and consequently that a register would be obtained of its maximum dilatation: but the difficulty consisted in applying the heat so equally that one part should not melt before another. The arrangement which I finally adopted to secure this purpose, and which was found to answer perfectly, was as follows. In the laboratory of the Royal Institution there is an excellent wind-furnance, from which proceeds a lateral horizontal flue, along which a flame may be drawn with any required degree of force. Into this flue open two muffle-holes, which give a complete view and command of the interior. From the equality of the draught, regulated by a register, the whole of this chamber may be kept at a low red, or an intense white, heat, by a proper management of the fuel in the body of the furnace.

The registers of the pyrometer were prepared for the experiment by drilling three holes on their under sides, communicating with the cavities in which the bars were placed; one at each extremity, and one in the centre. This was done for the purpose of allowing a vent for the melted metal, and to afford some criterion of the equality of the heat, by the time at which the metal ran from the different apertures. When the bar was properly adjusted in the register, it was carefully placed in the hot air-chamber, in a horizontal position, supported at each end by a small piece of brick, at a proper distance from the body of the fuel, accordingly as a greater or less degree of heat was required. The muffle-holes were then closed with their stoppers; all but a narrow slit, through which the progress of the heating and the flow of the metal could be observed. The equality of the heat could be very accurately ascertained by the uniform colour of the register as it became red; and any irregularity could easily be corrected by advancing one or other end more towards the fuel. In this manner I succeeded in obtaining very satisfactory results; except in the case of gold; and this metal requiring for its fusion rather more heat than I could at the time command in

the air-chamber, I laid the register upon the fuel in the body of the furnace, and it thus became only partially melted, and half the bar remained in the solid state. The amount of the expansion indicated is therefore evidently deficient, and must be discarded from the table. A similar accident happened once with brass; but this I have been able to rectify by subsequent trials.

I shall now arrange the results of my experiments in two tables:—the first showing, in arcs of the scale, the expansion of pure metals from 62° Fahr. to 212°, 662° Fahr., and their respective melting points; and the second exhibiting the expansion of certain alloys to the same points.

The bars were in all cases of the same length of 6·5 inches.

Table XIII.

Showing the progressive Expansion of the following pure Metals to their Melting Points.

From 62°	to 212°	to 662°	to Melting Point.
Tin	0° 55′	......	2° 30′
Lead	1 33	......	6 17
Zinc	1 40	5 50?	8 44
Silver	0 59	4 9	13 45
Copper	0 45	4 10	16 0
Gold	0 35	3 11	(7 51 not correct)
Cast iron	0 29	2 25	9 47

Table XIV.

Showing the progressive Expansion of the following Alloys to their Melting Points.

From 62°	to 212°	to 662°	to Melting Point.
Brass. Common	0° 54′	4° 42′	(8° 41′ not correct)
Brass. Copper $\frac{3}{4}$, Zinc $\frac{1}{4}$	1 9	4 51	13 39
Brass. Copper $\frac{1}{2}$, Zinc $\frac{1}{2}$	1 27	5 3	15 34
Bronze. Copper $\frac{15}{16}$, Tin $\frac{1}{16}$	0 52	3 37	9 49
Bronze. Copper $\frac{7}{8}$, Tin $\frac{1}{8}$	0 54	4 11	10 16
Bronze. Copper $\frac{3}{4}$, Tin $\frac{1}{4}$	0 58	4 44	10 55
Bronze. Copper $\frac{1}{2}$, Tin $\frac{1}{2}$	1 0	4 7	4 7?
Pewter. Lead $\frac{4}{5}$, Tin $\frac{1}{5}$	1 5	......	2 28
Type Metal. Lead and Antimony	1 5	......	3 13

The first remark which I shall make upon these tables regards the fusing points of the pure metals. Having ascertained for each the expansion due to certain definite increments of temperature, and the utmost expansion which they

undergo to their fusing points, it is clear that, had their expansion been equal for equal increments, we might have determined the true temperature of their melting points from these data. As it is, even, knowing something of the limits of error introduced into such a calculation by the increased rate of expansion at the upper part of the scale, and the direction in which it ought to affect the result, we may draw some important inferences with regard to the correctness of the determinations derived from other means. The following Table exhibits the results of such a calculation, compared with the melting points previously determined.

Table XV.

Fusing Points of Metals derived from their Expansions to 212° and 662° supposed equable.

	From 212° rate.	From 662° rate.	Real Temperature.
Tin	471°	......°	442° by Thermometer.
Lead	670	......	612 by Thermometer.
Zinc	848	960 ?	773 by Pyrometer.
Silver	2159	2049	1873 by Pyrometer.
Copper . . .	3262	2366	1996 by Pyrometer.
Cast iron . .	3096	2489	2786 by Pyrometer.

Now by these results, the accuracy of the pyrometer may, again, be placed beyond doubt, in a manner which was perfectly unforeseen at the time of instituting the experiments.

In the first place we have two metals, tin and lead, whose melting points being within the temperature of boiling mercury, have been accurately determined by the common thermometer. Upon calculating the same points from their several expansions to boiling water, measured by the pyrometer, upon the supposition that they maintain the same rate to their points of fusion, the temperature of the first comes out 29°, and of the second 58°, higher: that is to say, the rate of expansion of these two metals increases with the increase of temperature, as has been found to be the case with platinum, iron and copper, by the experiments of MM. Dulong and Petit. It is worthy of remark, that this increased rate in tin is equivalent to 29° in about 200°, and in lead to 58° in about 400°, above the boiling point of water. These results therefore indicate a very close agreement between the thermometer and pyrometer.

2ndly. The melting point of the next metal, zinc, is one of those which has been determined by immersion of the pyrometer into it, when it was in the act of fusion. Its temperature, so determined, falls short of the same point, calculated

from the expansion supposed equal, by 75°. This again indicates an expansion increasing at nearly the same rate (75° in 560°), as in the preceding instances of tin and lead. I pass over at present the result obtained by calculating from the expansion to the boiling point of mercury, as it presents an anomaly upon which I shall presently make some observations.

3rdly. The melting point of silver, determined in the same way by immersion, differs from that calculated from expansion in the same direction; and the difference (286° in 1660°) is nearly in the same proportion. The calculation from the rate of expansion to the boiling point of mercury comes much nearer to the melting point directly determined, and only differs from it 176°: proving that the rate of expansion increases with the increasing temperature.

4thly. A similar comparison instituted with copper presents us with a rate of expansion increasing much more rapidly than in the preceding instances; so that the melting point, calculated from the expansion to boiling water, differs from the true melting point no less than 1266°. Taking the rate of expansion to boiling mercury, the difference is reduced to 370°. And here again I may refer to the experiments of MM. Dulong and Petit in confirmation of the result; for they found that the temperature indicated by the expansion of a rod of copper was 50° Fahr. higher than the true temperature at 572° Fahr.

5thly. The interesting nature of the results which I obtained with iron, and the peculiar difficulties in arranging the experiments from which they were derived, will, I trust, excuse my entering more into their details than I have thought necessary in the preceding instances. I have already given the expansion of wrought-iron to the temperatures of boiling water and boiling mercury, and shown that the measures obtained with the pyrometer agree essentially with those determined by very different means by MM. Dulong and Petit. I have also proved that the melting points of gold and silver, determined by the expansion of the same bar of iron, agreed very closely with the same points determined by the expansion of platinum. I was extremely anxious to complete this series of experiments by measuring the expansion of iron to its melting point. For this purpose I had a small bar of iron cast from the best gray iron, and afterwards cleaned of all oxide and reduced to the size of the other bars employed by filing. Upon measuring its expansion to the temperatures of boiling water and boiling mercury, I found the arcs upon the scale respectively 0° 29′ and 2° 25′; and this being con-

siderably less than what I had obtained with the bar of wrought iron, I repeated the experiment with the latter in the same register that I had employed for the former, and obtained the measures of 0° 35′ and 2° 44′—nearly agreeing with the previous determination: so that there can be no doubt that cast iron expands less than wrought iron, though the rate of increase for the higher temperature appears to be the same in both.

[To be continued.]

XLIV. *Notices respecting New Books.*

Life Tables founded upon the Discovery of a Numerical Law regulating the Existence of every Human Being, &c. By T. R. Edmonds, B.A., *late of Trinity College, Cambridge.*

IN this work, of which we have been favoured with a copy, the author announces the discovery of a *new law of mortality*, which he has applied to the construction of a considerable number of annuity and other tables. The announcement in the title-page, that the tables are founded upon the *discovery*, instead of *the law*, is one of those inaccuracies of expression which occur in other parts of the book, and which bespeak carelessness at least on the part of the author, in the construction of his sentences.

The *new law* is thus announced.—During the succession of years and moments of life, the continuous change in the force of mortality is subject to a *very simple law*, being that of *geometric proportion*. But instead of *one* uniform progression, there are *three* distinct orders, corresponding respectively to infancy, manhood, and old age. The common ratios of the three geometric series are, we are told, *fixed and immutable for all human life in all ages of the world*. They are also said to be *now first discovered*: but it is not stated by what process, nor is it at all important to inquire; for they appear to be wholly empirical, and the tables founded upon them are of no practical value whatever.

Indeed this is the opinion of the author himself. For in page xii. he says, with great truth, that "in *all classes* of a population the mortality is *continually varying*." Hence the *new fixed* and *immutable law* cannot be applicable, except at some particular moment, to any of such classes. The author afterwards very justly remarks, that "to generalize from a single fact is absurd; and it is an absurdity of this kind into which those people fall who would apply observations made on *one kind* of life to *all kinds* of life." A remark which implies an equal absurdity in applying his own general law, which is *fixed for all human life*, to any one class or condition of mankind,—or in other words, to any one practical purpose.

But although we concur with the author in the uselessness of his own tables, we dissent from most of his opinions relative to the comparative value of others.

In page xii. he appears to prefer the Northampton Table, chiefly *because it is supported by the name of Dr. Price.* But the known incorrectness of this table, or, as our author is pleased to express himself, the "*slight* inaccuracy of its adjustment of mortality to each age," is not, in his judgement, of any "sensible value in practice;" yet he afterwards admits that "its applicability to the British population of the present day may fairly be questioned." We believe that the Northampton Table is not capable of affording any accurate measure of life contingencies of any kind; in which respect it so much resembles our author's own offspring, that we are not surprised at its having received from him a kind of fatherly affection.

The Government Table, on the contrary, deduced from the lives of English annuitants, because, as the author says, it "opposes *my theory*, as well as *that of every other person*", incurs his severe displeasure. A clearly demonstrable fact opposed to a favourite theory is, we admit, vexatious enough, and more particularly so when the theory is one of our own invention, one of our first-born bantlings, and one upon which our hopes of reaping a full harvest of renown has been anxiously founded. So harassing indeed has this opposition of the Government Table to our author's theory been to his feelings, that he would hurl the Table and its author to perdition together, with perhaps the printer and his devils into the bargain.

His method, however, of disposing of the author is not marked by that *precision* which we should have expected from a B.A. of Trinity College, Cambridge. He says, "the *reported mortality* of English annuitants is not entitled to much confidence," because it "rests upon the *authority of a person* whose qualifications for the task undertaken are unknown to the public." But the *reported mortality* is that which appears in the records preserved in the Government offices, and does not rest upon the authority of any person except as a transcriber, whose only requisite qualifications are, that he should be able to read and write, and, as school-boys term it, do a sum in Addition. We will, however, deal fairly with Mr. Edmonds, and not pin him down to his own loose expression, "*reported mortality*," but will help him to a phrase, which if any distinct meaning pervaded his mind when he wrote the passages we have just quoted, may perhaps represent that meaning. He possibly intended to say that the reported *probability of life*, among the English annuitants, is not entitled to much confidence, because the qualifications of the person deducing it are *unknown to the public.*

Now, of all the reasons we have ever heard for discrediting the result of a rather complicated arithmetical process, this is the most futile and absurd:—because the qualifications of the author are *unknown to the public.* Why upon this principle, the Tables of Mr. Edmonds might, even if they were really good for anything, flap their leaden wings to the end of time without attaining even the lowest degree of public confidence. For what do the *public* know of this gentleman's qualifications? But we will ask him what he means by *the public?* We much suspect that in his vocabulary it signifies only the individual occupant of his own chambers. We shall, however, construe the phrase according to its ordinary acceptation, and shall

suppose *our* public divided into three classes: the first consisting of those who from personal acquaintance with the author of the Government Tables have had an opportunity of ascertaining his qualifications for the task of framing them; the second, those who have read his Report, printed by order of the House of Commons, March 31, 1829, and are capable of forming a judgement of the author's fitness from that report; and the third, those who neither know nor care any thing about such matters. Now we have not the least hesitation in affirming, that the first two of these classes do know the competency of the author to perform his task; and in confirmation of this assertion, we shall quote an authority which we believe Mr. Edmonds himself will not dispute. In a paper by Mr. Lubbock, in the third volume of the Cambridge Philosophical Transactions, and in p. 330, Mr. Edmonds will find the following passage: "Mr. Finlaison has very recently published extensive tables of mortality, formed from the Government tontines and annuitants, which are rendered equally valuable by the accuracy of the materials from which they have been deduced, and the very great care and attention which has been bestowed on them by the author." After this, we apprehend we should only waste the time of our readers by pursuing this subject further.

We have already expressed our dissent from many of the doctrines and opinions of this author: there are, however, some in which we readily concur; as those in which we are taught that "*good air is as necessary to health as good food*," and that "*the increase of a population has a great dependence upon the number of women at the child-bearing age*," and others equally conspicuous for their truth, and their beautiful simplicity, as general laws.

We must now limit ourselves to a very few more observations. Our author says, p. xxi. "The check on the *exertion* of the prolific power is scarcity of food." We apprehend his meaning to have been, that scarcity of food abates the prolific power. But let him look to Ireland, with its scanty means of subsistence, and its overflowing population; and he may also discover, when he sets about observing facts instead of building theories, that the largest families are upon an average produced by the poorest classes.

We are told, p. xxx. that "the circumstances most favourable to *vitality* consist in alternations of privation and saturation:"—to starve one day, and feed to repletion on the next. We, however, seriously recommend our author, and particularly at this time, not to trust his own vitality to such an experiment.

And in p. viii. we have the comfortable doctrine announced, that "the hopes of an *indefinite* prolongation of the term of human life have now ceased to be visionary," and this, we presume, without the assistance of the Hermetic philosophy.

We suspect that "too much learning" has exercised an unwholesome influence on the mind of our author, who is probably young, with an active and uncurbed imagination, which he has permitted on this occasion to hurry him into as many scrapes as the wild steed of Mazeppa did his unwilling rider. But although we have thus amused ourselves, and perhaps our readers, at the author's expense, we can assure him that we do not entertain the slightest un-

friendly feeling towards him; and although an entire stranger to us, we wish him every success in his laudable endeavour to build up an honourable reputation for himself: yet we earnestly counsel him not again to attempt the destruction of that of his neighbour; as it betrays both bad taste and bad feeling, and may eventually convert into personal enemies those who might otherwise become useful friends.

On Suspension Bridges; containing an Inquiry into the proper Forms of their Catenaries; with Remarks on the Menai Bridge, and that at Broughton; as likewise some Account of the Failure of the latter. By EATON HODGKINSON, *Manchester*, 1831.

In the eastern parts of the world, rope and chain bridges of large span have for a long period been in use; but in Europe the adoption of bridges of suspension is of comparatively modern date, and has opened a new and interesting field for inquiry and experiment, both for the engineer and the mathematician, and has rendered of practical importance the theory of the catenarian curve. During the last century, mathematicians investigated many of the properties of this curve; but the addition of the materials which are necessary to form the roadway, and to insure sufficient strength for the variable and large loads of transit, have added new data to the problem.

Mr. Eaton Hodgkinson has, in the present treatise, given a very clear abstract of the properties of this curve under all the probable variations it is liable to in its application to bridges of suspension; not only when the substance of the chain is of uniform strength, but also when the strength varies as the strain,—concluding with an example upon assumed data.

The neat and elegant manner observed throughout renders the tract a desirable object in the library of the practical engineer.

The second part of the work contains an account of the chain bridge at Broughton, near Manchester, with a particular estimate of the strain upon the various parts as compared with the strength, and also some important information on the form of the joints of the links; to which are added some observations on high tests, and on defective welding of the bars.

Next follow a few remarks on the Menai Bridge, showing that it possesses sufficient strength to support seven times its own weight.

An Appendix is given, containing remarks and observations on the cause of failure of the bridge at Broughton, after standing some years, but which has since been repaired and made more secure*.

Theoretical and Experimental Researches to ascertain the Strength and best Form of Iron Beams. By the same Author.

The builder and engineer will find in this treatise many important experiments, conducted with great skill, and described with accuracy of detail. It should be read by every person who intends to use iron beams. The author has long been known for his abilities as a mathematician, and his application of those abilities to practical purposes.

* An account of the fall of the Broughton Suspension Bridge, with some particulars of the causes of its failure, were given in Phil. Mag. and Annals, N.S. vol. ix. p. 384.

The use of iron for beams has been lately much and deservedly resorted to. In the application of a new material it is to be expected that there will be occasional failures, partly owing to motives of œconomy, and partly to want of skill in the builder. The price of iron renders it desirable that the smallest quantity should be used which is consistent with safety and durability.

There are several popular treatises on the strength of iron and the form of beams; but the rules contained in them are frequently at variance with experiments, and therefore mislead the practical man, who has not leisure or ability to investigate the principles upon which they are founded.

In this tract, the author investigates the theory of strength, and of resistance to fracture and deflection, and supplies by judicious experiments the defective elementary data, giving the particulars at large of numerous experiments on iron of various forms and dimensions, so as to enable a practical builder to satisfy his own mind of the ground upon which the deductions are made. These experiments prove that the form recommended by the late Mr. Tredgold is inferior to others which have since been adopted, and also that the formula given by Mr. Tredgold for determining the strength is incorrect, and may lead to serious errors*.

Much of the work is occupied by the subject of the transverse strength and strain, and some useful deductions are made on the ultimate deflection,—a point which at present deserves further investigation.

The author acknowledges his obligations to Messrs. Fairbairn and Lillie for the assistance they rendered him at their foundry, by which he was enabled to adopt a scale of dimensions seldom within the means of a theoretical investigator. Instances of liberality of this kind are frequently met with in this country, much to the honour of the persons who thus manifest themselves friends to their country and to science.

The result of Mr. Hodgkinson's experiments on cast iron beams having the bottom rib containing more than half the matter of the whole beam, shows that the breaking weight was proportionate to the area of the bottom rib, and to the full depth of the beam, and inversely as the length, subject to a constant factor depending on the position of the beam in the casting, the vertical castings being about $\frac{1}{24}$th stronger than the horizontal castings; the quality of the metal will also modify the factor in some degree. In such important experiments as these, the determination and statement of the specific gravity and hardness, together with the modulus of elasticity of the material, would add to their value, with very little additional burden to the operator.

On the Economy of Machinery and Manufactures. By CHARLES BABBAGE, *Esq. A.M., Lucasian Professor of Mathematics in the University of Cambridge, and Member of several Academies.* London, 1832.

Although the present volume does not properly come within the sphere of a scientific Journal, yet the principles which it discusses

* Analyses of the first and second editions of Mr. Tredgold's Essay on the Strength of Cast Iron, will be found in Phil. Mag. vol. lx. p. 137; and vol. lxiii. p. 52.

are so intimately connected with the progress of the scientific arts, and the operations which it describes are so essential to the perfection of scientific instruments and scientific machinery, that we feel it a duty to make our readers acquainted with the merits of so remarkable a work. The name of the author, indeed, is sufficient to attract the attention of scientific men to any work which emanates from his pen; but those who are acquainted with Mr. Babbage's merits as an inventor, and have acquired any knowledge of the nature and power of that extraordinary machinery which he has taught to perform the most complicated calculations, will turn with intense interest to a volume containing an account of the various resources of the mechanical arts which the author has himself studied in the different workshops and factories of Europe, and a classification of the modes of action of tools and machines, and a generalization of the principles of their application to supersede the labour of the human arm.

Mr. Babbage's work is divided into *two* sections. The first section contains a view of the mechanical part of the subject, which occupies *twelve* chapters. The *first* chapter treats of the general sources from which the advantages of machinery are derived; and the *nine* following chapters treat of principles of a less general character, such as, Accumulating power,—Regulating power,—Increase and diminution of velocity,—Extending the time of action of forces,—Saving time in natural operations,—Exerting forces too great for human power, and executing operations too delicate for human limits,—Registering operations,—Œconomy of materials employed,—and Of the identity of the work when it is of the same kind, and of its accuracy when of different kinds. The *eleventh* chapter treats of Copying, and is divided into Printing from cavities,—Printing from surface,—Copying by casting,—Copying by moulding,—Copying by stamping,—Copying by punching,—Copying with elongations,—and Copying with altered dimensions. This chapter, which is an exceedingly popular and interesting one, is full of the most curious practical information, and contains the first account that has yet appeared of Mr. John Bate's ingenious art of Engraving from Medals. The *twelfth* chapter, which terminates the first section, treats of the method of observing manufactures, and deserves the peculiar notice of the scientific traveller.

The *second* section of the work begins with an introductory chapter *on the difference between making and manufacturing**, and in *eighteen* succeeding chapters, contains a discussion of most of the questions and principles which belong to the political œconomy of manufactures, such as, The influence of verification on price,—The influence of durability on price,—On price as measured by money,—On raw materials,—On the division of labour,—On the division of mental labour,—On the separate cost of each process,—On the causes and consequences of large factories,—On the position of great factories,—On overmanufacturing,—Inquiries previous to commencing any manufactory,—On contriving machinery,—On the application of machinery,—On the duration of machinery,—On combinations amongst masters or workmen against each other,—On combinations of masters against the

public,—On the effect of taxes and local restrictions on manufactures,—and On the exportation of machinery. The work is then concluded by the *thirty-second* chapter, a piece of powerful and eloquent writing, which treats of the future prospects of manufactures as connected with science.

Having thus given our readers a correct outline of the various subjects treated of by Mr. Babbage, we shall select some specimens of the interesting information which this volume contains.

In treating of the inquiries which it is necessary for the projector of a new manufacture to make respecting the quantity of the article likely to be consumed, Mr. Babbage gives the following happy and interesting example, given in evidence before the House of Commons, by Mr. Osler, a manufacturer of glass beads and other toys of the same material, at Birmingham.

"Eighteen years ago," said Mr. Osler, "on my post journey to London, a respectable-looking man, in the City, asked me if I could supply him with dolls' eyes. He took me into a room quite as wide, and perhaps twice the length of this, and we had just room to walk between stacks, from the floor to the ceiling, of parts of dolls. He said, 'These are only the legs and arms; the trunks are below:' but I saw enough to convince me that he wanted a great many eyes; and as the article appeared quite in my own line of business, I said I would take an order by way of experiment; and he showed me several specimens. I copied the order. He ordered various quantities, and of various sizes and qualities. On returning to the Tavistock Hotel, I found that the order amounted to upwards of 500*l.* I went into the country and endeavoured to make them. I had some of the most ingenious glass toymakers in the kingdom in my service; but when I showed it to them, they shook their heads, and said they had often seen the article before, but could not make it. I engaged them by presents to use their best exertions; but after trying, and wasting a great deal of time for three or four weeks, I was obliged to relinquish the attempt. Soon afterwards I engaged in another branch of business (chandelier furniture), and took no more notice of it. About eighteen months ago I resumed the trinket trade, and then determined to think of the dolls' eyes; and about eight months since I accidentally met with a poor fellow who had impoverished himself by drinking, and who was dying of consumption, and in a state of great want. I showed him ten sovereigns, and he said he would instruct me in the process. He was in such a state that he could not bear the effluvia of his own lamp; but though I was very conversant with the manual part of the business, and it related to things I was daily in the habit of seeing, I felt I could do nothing from his description. He took me into his garret, where the poor fellow had œconomized to such a degree, that he actually used the entrails and fat of poultry from Leadenhall Market to save oil. In an instant, before I had seen him make three, I felt competent to make a gross, and the difference between his mode and that of my own workmen was so trifling, that I felt the utmost astonishment.

"As it was eighteen years ago that I received the order I have men-

tioned, I took the present reduced price of dolls' eyes; and calculating that every child of this country not using a doll till *two* years old, and throwing it aside at *seven*, and having a new one annually, I satisfied myself *that the eyes alone would produce a circulation of a great many thousand pounds.* I mention this merely to show the importance of trifles, and to assign one reason amongst many for my conviction that nothing but personal communication can enable our manufactures to be transplanted."—pp. 199—201.

Mr. Babbage mentions a very instructive example of the difficulty of estimating the effects of a machine, and of the ingenious way in which the difficulty was overcome. In order to fix a proper toll for steam carriages, a Committee of the House of Commons endeavoured to ascertain, from competent persons, the injury done by the atmosphere to a well-constructed road, and then the proportional injury which the same road sustained from horses' feet and from wheels.

"Mr. Macneall," says Mr. Babbage, "as superintendent, under Mr. Telford, of the Holyhead roads, proposed to estimate the relative injury from the comparative quantities of iron worn off from the shoes of the horses, and from the tire of the wheels. From the data he possessed respecting the consumption of iron for the tire of the wheels and for the shoes of the horses, of one of the Birmingham day coaches, he estimated the wear and tear of roads arising from the feet of the horses to be three times as great as that arising from the wheels. Supposing repairs amounting to 100*l.* to be required on a road travelled over by a fast coach at the rate of *ten* miles an hour, and the same amount of injury to occur on another road used only by waggons moving at the rate of *three* miles an hour, Mr. Macneall describes the injury in the following proportions:

Injury arising from	Fast Coach.	Heavy Waggon.
Atmospheric changes ..	20	20
Wheels	20	35·5
Horses' feet drawing ..	60	44·5
Total injury..	100	100

One of the results of these experiments is, that every coach which travels from London to Birmingham *distributes about* eleven *pounds of wrought iron along the line of road between these two places.*"—pp. 201—203.

In treating of the effect of taxes upon manufactures, Mr. Babbage has entered very briefly upon the subject of Patents,—a subject peculiarly connected with that of his work, and to which he should have allotted more space, and done greater justice. In the only paragraph which relates to the present state of the law of patents, Mr. Babbage remarks that

"It is clearly of importance to preserve to each inventor the sole use of his invention, until he shall have been amply repaid for the risk

and expense to which he has been exposed, as well as for the talent he has exerted. But the varieties in the degrees of merit are so numerous, and the difficulties of legislating upon the subject are so great, that it has been found almost impossible to frame a law which should not, practically, be open to the most serious objections."—p. 289.

The difficulty of framing a perfect law of patents, which shall reconcile all opposing interests, is undoubtedly great; but the present law is so disgraceful in its character, so injurious to the revenue of the country, so subversive of the rights, and so destructive of the property of inventors, that any change upon it must be an improvement. Let the reader only cast his eye over the following table of the expense and duration of patents in the different kingdoms of Europe and America, and then ask himself what he thinks of English legislation.

Countries.	Expense of Patents.	Duration of Patents.
Great Britain and Colonies*	355 0 0	14
America	6 15 0	14
France......................	12 0 0	5
	32 0 0	10
	60 0 0	15
Netherlands	6*l.* to 30*l.*	5, 10, 15
Austria	42 10 0	15
Spain, Inventor	20 9 4	15
—— Improver	12 5 7	10
—— Importer	10 4 8	6

Great Britain thus robs every poor inventor of 355*l.* even if he never derive a farthing from his invention! Laws which thus tax genius, like those which tax knowledge, ought not to be allowed a single day's existence.

Although the few extracts which we have made from Mr. Babbage's volume are in themselves highly interesting, yet they convey no idea of the multifarious and popular subjects which are treated of in the work before us, which may be read with as much pleasure as instruction by persons of all ages and all conditions in society. It is, indeed, one of those few works which are equally fitted for the perusal of the philosopher and the general reader.

Mr. Babbage possesses the happy art of clothing the stores of his highly endowed mind with the richest drapery of language. In description he is perspicuous, in argument he is concise, and in general views of the past, as well as in his anticipations of the future, he rises into a strain of chaste eloquence, in which he has few rivals. The following beautiful passage, which concludes the book, will, we are sure, justify these observations.

"In whatever light we examine the triumphs and achievements of our species over the creation submitted to its power, we explore new sources of wonder. But if science has called into real existence the visions of the poet,—if the accumulated knowledge of ages has

* Viz. 120*l.* for England; 125*l.* for Ireland; 100*l.* for Scotland; and 10*l.* for the colonies.

blunted the sharpest, and distanced the loftiest of the shafts of the satirist, the philosopher has imposed on the moralist an obligation of surpassing weight. In unveiling to him the living miracles which teem in rich exuberance around the minutest atom, as well as throughout the largest masses of ever-active matter, he has placed before him resistless evidence of immeasurable design. Surrounded by every form of animate and inanimate existence, the sun of science has yet penetrated but through the outer fold of nature's majestic robe; but if the philosopher were required to separate from amongst those countless evidences of creative power, one being the master-piece of its skill; and from that being to select one gift, the choicest of all the attributes of life;—turning within his own breast, and conscious of those powers which have subjugated to his race the external world, and of those higher powers by which he has subjugated to himself that creative faculty which aids his faltering conceptions of a Deity,—the humble worshiper at the altar of truth would pronounce that being, man; that endowment, human reason.

"But however large the interval that separates the lowest from the highest of these sentient beings which inhabit our planet, all the results of observation, enlightened by all the reasoning of the philosopher, combine to render it probable that, in the vast extent of creation, the proudest attribute of our race is but, perchance, the lowest step in the gradation of intellectual existence. For, since every portion of our own material globe, and every animated being it supports, afford, on more scrutinizing inquiry, more perfect evidence of design, it would indeed be most unphilosophical to believe that those sister spheres, glowing with light and heat, radiant from the same central source—and that the members of those kindred systems, almost lost in the remoteness of space, and perceptible only from the countless multitude of the congregated globes—should each be no more than a floating chaos of unformed matter;—or, being all the work of the same Almighty Architect, that no living eye should be gladdened by their forms of beauty; that no intellectual being should expand its faculties in deciphering their laws."

Comparative Account: Population of Great Britain. Ordered by the House of Commons to be printed, 19th October, 1831*.

The scientific world is indebted to Mr. Rickman for superintending four returns of the population of Great Britain, in 1801, 1811, 1821, and 1831,—a long period for the same individual to be occupied with the same subject, and which he has conducted with all the ability that a long familiarity with so complicated an inquiry demands, and with all the scrupulous accuracy which an ardent and philosophic disciple of this peculiar kind of arithmetic could display.

There is nothing, it will be admitted, about the subject itself, considered as mere dry details of figures, that particularly invites attention; and it is rather in its accessories and in the well-springs of philosophic light that its conclusions supply, that we must look for

* The *Comparative Account* has been just published in a separate form, (price 10*s*.,) in which it cannot fail to have an extensive circulation.

those motives of encouragement which could have led an accomplished mind, for so long a period, to devote its best energies to an inquiry, which by the greater part of mankind is looked upon in a pre-eminent degree as most tedious and uninviting. The English people too, with a feeling almost peculiar to themselves, regard all statistical inquiries with a singularly jealous eye; and the sight of a parish officer at the door is apt to awaken all the disagreeable feelings connected with taxation and tithes. Nor is the parochial overseer himself a willing agent in any undertaking of the sort; since he shrewdly suspects, notwithstanding the clear and explicit nature of the instructions he receives, that the Government may have some dark and mysterious purposes in reserve, for wishing so exactly to know the precise amount of males above twenty years of age,—what houses are inhabited and building, how many are unoccupied, or inhabited only by "the spider and the owl";—how many labourers are employed in the quiet and tranquil pursuits of agriculture;—how the different grades of the whole living community of man are distributed throughout the great social edifice,—and what are the measures of the great springs of human activity;—all these *he* thinks are wanted in order that future ministers may draw from them the sinews and *materiel* of war.

With the great amount of these difficulties and prejudices, drawn together from the Orkneys to the Land's End, and in all the intensity and extent of the varied feelings which can possibly influence a busy and inquiring community like that which peoples the soil of Great Britain, Mr. Rickman must have had to contend, during the long course of years in which he has been following up, in all their generality, the tedious and varied details of this complicated inquiry; and return after return must have been sent back for correction into the Wapentakes of York, the Rapes of Sussex, or the Hundreds of Devon, before accuracy was completely obtained; nor have our own Scottish schoolmasters been entirely exempted from the very grave charge of carelessness and blunder. Now where, it may be asked, can a mind fitted by nature to grapple with higher and far loftier pursuits, seek for its proper reward when pursuing a subject of this nature, but in the elevated consciousness of devoting its energies to its country's good? The philosopher who loves to dwell on causes and effects,—to trace the deep processes of thought by which the great purposes of nature have been revealed, both in the heavens above, and in the physical structure of the earth on which he treads;—or who endeavours to deduce from incongruous masses of figures results closely interwoven with the social destiny of man, and the mysterious laws which seem to regulate his progression here,—cannot undervalue labours like these. He will appreciate, and that highly, that ardent and profound devotion to one inquiry, which has disclosed during the lapse of thirty eventful years so many beautiful and important results, tending to illustrate more perfectly the statistics of his own great country. He will mark also with peculiar satisfaction,—and he will not fail, in the deep and durable feelings of his heart, to congratulate Mr. Rickman upon the fact,—how each succeeding census has become more accurate than its predecessor; and what high probabilities have been opened of our eventually obtaining a greater and far more accurate body of

statistical knowledge than is possessed by any other people; and how the whole country, at the last enumeration,—overseers, farmers and manufacturers, awakened as if by inspiration to a sense of the importance of statistical researches,—rather greeted than avoided the inquiry; and how, for the first time, a great majority of parish officers became absolutely enamoured with population returns;—no longer shrinking from the undertaking as one affording no useful results *to them*, but looking to it as a measure in which their own individual welfare was most intimately concerned.

Mr. Rickman has had the pleasure, apart from all political considerations, of seeing this very great change brought about in the public mind; and to him the alteration cannot but have proved satisfactory. No longer doomed to see noble peers and wealthy commoners regarding with indifference the population volume, he now finds the returns sought after on all sides; and the boundaries of boroughs, the limits of *Ainsteys*, and the numerical amounts of their inhabitants, forming a keen subject for senatorial debate. A permanent taste for statistical inquiries may thus spring out of the great question of Reform; and men who in times past looked at the data or operations of the census with indifference or with suspicion, have now been aroused to a sense of its importance and value.

A census of the people philosophically conducted is a truly great and magnificent object of inquiry;—and who can tell the light its varied details will ultimately throw on the whole frame-work of society? From its high and lofty places, downward through all its gradations, to the lowest shades of poverty and vice, what a singularly curious and interwoven fabric! Who at the present moment can form even a conjectural notion of the great social edifice?—What are its workings, and the springs which give it life; what the causes which make its entire structure exhibit at one time all the fair and florid forms of health, and in another all the unhappy characteristics of vicissitude and decay;—which in one season beams with hope, while tranquillity and joy are found within its borders, but at another exhibits a feverish anxiety,—dark and unkindly suspicions rising angrily against its best benefactors, and destroying the very hand that is raised to comfort and support it? Such is human society; and to illustrate its nature completely is the final object the political arithmetician has in view. It is not for the mere pleasure of congregating dense piles of figures, as some have supposed, that all these efforts have been made by the cultivators of statistics to gather together from every region numerical results, but to endeavour in the end to deduce from them conclusions bearing as well on the moral as on the physical condition of man. The two are mysteriously but beautifully interwoven; and while the philosopher approaches a problem of so lofty and elevated a kind, with all the timidity which its vastness and magnitude inspires, he feels that he is treading on sure and perfect ground, when he is patiently gathering together the great numerical foundations of statistical science. Nor is he at all disheartened by the consideration, that he is only preparing the tools which other artificers are to use; and while he can never hope to see the inquiry perfected in all the

generality which his noble imagination has conceived, he nevertheless consoles himself with the glorious but substantial prospect before him; nor will he relinquish his labours, whatever be the amount of the obstacles which beset him.

There are persons, however, distinguished in other matters for a fair share of sagacity, who gravely doubt the propriety of these repeated enumerations, and who now and then throw out dark and unkindly hints respecting their accuracy. Such individuals, it would seem, form but limited and imperfect views of the condition of man, and have no notion that the necessity for more accurate statistical results increases exactly in proportion as population advances, and as the basis of civilization is widened. They forget that the numerical amount of a people enters very largely into many important considerations; and that the necessity for contemplating statistics on a wider and much more liberal basis than has hitherto been done, has been prodigiously increased, even within a comparatively short time. The condition of man in almost every region of the globe is changed, and we have only to look around us in the narrowest circle of the community, to behold elements and principles in action, of whose existence a few years ago we had no conception. There are impulses on an immense scale impelling population forward;—artificial wants of a new and unthought-of kind continually creating; and the basis of the great social system is widening and spreading out into innumerable forms on all sides around. The "great constants"—to adopt an idea of Mr. Babbage—which mark these important changes, it is the business of statistics to collect; and we say therefore, and we say it strongly, that so far from our permitting the slightest damper to be thrown on these interesting labours, it should rather be our object to cherish and extend them; that no zeal can be too ardent, no industry too intense for its prosecution; and that while we have before us such splendid memorials of perseverance and skill, the mode by which these admirable arrangements have been fulfilled should be cherished and preserved. Every succeeding census will thus be rendered more perfect than its predecessor; each successive step of the great statistical ladder will conduct us into a region more accurate and refined; and though the present generation may not reap all the fruits of Mr. Rickman's labours, our successors will, we trust, regard them as the foundation stones of that mighty pyramid of knowledge, which it will be the duty of succeeding generations to raise; and that when its future history shall be written, the indefatigable labourer who contributed so effectually to secure its basement courses, may have his name engraved in deep and imperishable characters upon them.

We wish Mr. Rickman could be prevailed upon to give us a short history of the operations of each successive census; what obstacles were opposed to its execution, and how these impediments were overcome; what other difficulties remain to be conquered; and above all, an account of the machinery which he himself has employed for digesting the multifarious returns;—what has been the amount of labour expended on each population volume, and what further views his enlarged experience could suggest for rendering the future operations of

the census more perfect and more complete. The analysis of the various divisions of labour and the classification of the computers, under the able management of M. Prony, forms one of the most interesting and instructive features of the great French logarithmic tables. But we must hasten to the consideration of other subjects.

The four successive Acts of Parliament relating to the important subject of the census of the people have been in some respects similar, whilst in others they have been subjected to considerable modifications. In the number of houses and persons, for example, they remain the same; but the Acts of 1811 and 1821 differed from that of 1801 in demanding the occupation of *families* instead of *persons*, and which led to the useful result that about one third of the population of Great Britain are employed in agriculture; rather less than one half in trade, manufacture and commerce, leaving to the third or miscellaneous class one fifth of the aggregate population. In the Act for 1831 another great improvement was made—by finding the number of males who have arrived at twenty years of age, their occupations being then usually settled for life; and it would appear that this alteration has been forced upon Mr. Rickman's attention by the remarkable statistical fact, that the particular age of twenty furnishes to a certain extent a ready test to the magistrates,—before whom the returns are authenticated,—of the accuracy of the enumerator, one half of the existing male population being thus included in the inquiry. On a great scale this approach to equality holds good to a remarkable extent, the males under twenty years of age being 3,072,392, and above that age 3,002,200.

The important question of the ages of persons, which succeeded beyond expectation in 1821, was not repeated in 1831, principally on account of its imposing too much labour in combination with the inquiries which the latter act embraced, relative to the various trades and occupations of the community. There must be a limit, it is manifest, to the objects of every Act. We cannot embrace everything, and if we attempt too much, the machinery will most assuredly break down. Legislation must be practical in all its objects. Every step made must be on sure grounds, and we must leave as little as possible to uncertainty and chance. If therefore in this census we embrace one object, in the next another may be substituted for it, and in some succeeding one the original clause may be restored. The objects of a census may thus be, like the terms of a recurring series, appearing and disappearing at periodical intervals, so that in a long lapse of time all the terms in their utmost fulness may be made to appear.

The inquiry relating to the trades and occupations of the people of England, (so diversified is human industry here,) cannot but lead to important results. A minute analysis of social life must open new and satisfactory views of the great and mysterious frame-work of society. Continued for a long course of time, it must disclose to the keen inquirer into the sources and influence of national wealth, a perfect view of social and moral organization. There is a relation of some kind, of a fixed and immutable nature, among the various grades of society, and there must be laws which govern the numerical amount

of millers, bakers, carriers, coal-merchants, boat-builders, iron-founders, paper-makers, printers, opticians, tailors, publicans, barbers, butchers, pastry-cooks, cow-doctors and chimney-sweeps, although we are unable at present to fathom the invisible principles which bind them together. Time may develop some of them, and being grounded on numbers derived from absolute population, the inquiry will no longer be the sport of uncertain speculation, but assume a definite and practical form. The only example yet afforded by Mr. Rickman is that which relates to the city of York; and when the whole population becomes subjected to a classification of this sort, the most satisfactory results must be anticipated. We subjoin this interesting document together with the schedule return of that ancient city.

City of York (Schedule Return).

	Houses.				*Occupations.*			*Persons.*		
Name and Description of Place.	Inhabited.	Families.	Inhabited Buildings.	Uninhabited Buildings.	Families chiefly employed in Agriculture.	Families chiefly employed in Trade, Manufactures, and Handicraft.	All other Families not comprised in the two preceding Classes.	Males.	Females.	Total of Persons.
City of York (31 Returns)	4,586	5,803	71	365	119	3,528	2,156	11,986	14,274	26,260
Ainstey of the same (35 do)	1,788	1,893	3	84	1,216	342	335	4,521	4,581	9,102

		Agriculture.								Male Servants.		
	Males 20 Years of Age.	Occupiers employing Labourers.	Occupiers not employing Labourers.	Labourers employed in Agriculture.	Employed in Manufacture, or in making manufacturing machinery.	Employed in Retail Trade, or in Handicraft as Masters or Workmen.	Capitalists, Bankers, Professional Men, and other Educated Men.	Labourers employed in Labour not Agricultural.	Other Males 20 Years of Age (except Servants).	20 Years of Age.	Under 20 Years.	Female Servants.
City of York.	6,378	23	63	176	222	3,601	512	1,113	495	173	73	1,619
Ainstey of the same	2,362	317	184	972	6	472	79	104	168	60	15	469

CITY OF YORK. Number of Males (Twenty years of Age) employed.

SPECIFICATION.

Animal Preserver 1
Artificial Flower-maker 1
Auctioneer, or Appraiser 13
Broker 14
Baker, Gingerbread, Fancy 56
Barber, Hairdresser, Hair-dealer 47
Basket-maker 16
Blacksmith, (Horse-shoes) 39
Nailer 3
Boat-builder, Shipwright..........
Caulker 3
Sail-maker............................ 1

Bookbinder 31
Bookseller or Vender 24
Brass-worker, Tinker 6
Brewer 29
Brush-maker 13
Builder 47
  Landjobber 6
  Bricklayer 118
  Brick-maker 20
  Lime-burner ....
  Plasterer 13
  Slater 6
  Mason or Waller 136
  House-painter 57
Butcher, Flesher 101
Carpenter 190
  Cabinet-maker 95
  Wheelwright 11
  Sawyer 31
  Saw-maker 1
  Plane-maker 17
Carrier, Carter 30
Carver and Gilder 19
Cheesemonger 8
Chemist and Druggist 54
Clock and Watchmaker 19
Clothier (Woollen) 6
  Linendraper, Haberdasher ... 75
  Silk-mercer or Dealer 3
  Calenderer 2
Coach-maker 25
Coach-owner, Driver, Grooms, &c. 88
  Horse-dealer, Stable, Hackney-coach or Fly Keeper 26
Coal-merchant, Fuel 28
Comb-maker 18
Cooper 24
Copperplate Printer, Engraver... 9
Cork-cutter 7
Corn-dealer 7
Currier 56
Cutler 11
Dyer 11
Drysalter, Colouring Materials... 2
Earthenware, China, Pottery ... 10
Farrier, Cow Doctor, Cattle Doctor 5
Feather-dresser 1
Fellmonger 28
Fish-dealer 13
Fruiterer 5
Furrier 2
Glass-blower 6
Glass-cutter 10
Glazier, Plumber 41
Glover 23
Grocer, Greengrocer 96
Gun-maker 9
Harness-maker, Collar-maker ... 9
  Saddler 28
  Saddletree-maker 1
  Whip-maker 5
  Bridle-cutter 1
Hatter and Hosier 18
Heckle-maker 2
Huckster, Hawker, Pedlar, Duffer 18
Ironfounder 17
Ironmonger 13
Jeweller 23
Lace-dealer 14
Maltster 9
Marble-cutter, Statuary 25
Milkman, Cow-keeper 59
Miller 34
Nightman, Scavenger 5
Old Clothes Dealer, Ragman 9
Optician 3
Organ-builder 1
Paper-maker 3
Paper-stainer 4
Pastry-cook, Confectioner 33
Patten-maker 8
Pavior 1
Pawnbroker 3
Pipe-maker 4
Poulterer 5
Printer 47
Printseller 3
Publican, Hotel or Inn-keeper, Retailer of Beer 181
Rope-maker 20
Shoe and Boot-maker or mender. 448
Shopkeeper, (Dealer in sundry necessary articles, such as are sold in a village shop)... 67
Silversmith 1
  Britannia Metal Smith 1
Soot and Chimney-sweeper 8
Spirit-merchant, Spirit-shop...... 13
Stationer 7
Stay-maker 2
Straw Plat and Bonnets 9
Tailor, Breeches-maker 224
Tallow-chandler, Wax-chandler 10
Tanner 18
Tea-dealer 34
Tinman 33
Tobacconist 15
Toyman 6
Turner 24
Undertaker of Funerals 3
Upholsterer 9
  Chair-maker 4

Umbrella-maker	1	Whitesmith	64
Wharfinger	5	Wine-dealer	13
		(122 Trades)	3,548

AINSTEY OF THE CITY OF YORK. SPECIFICATION.

Baker	1	Farrier, Cattle Doctor, Cow Doctor	2
Blacksmith, (Horse-shoes)	46	Glazier, Plumber	2
Brewer	3	Grocer	7
Bricklayer	17	Huckster, Hawker, Pedlar, Duffer	7
Brick-maker	18	Maltster	2
Mason or Waller	8	Miller	10
House-painter	1	Paper-maker	7
Butcher, Flesher	25	Publican or Inn-keeper, Retailer of Beer	37
Carpenter	30	Saddler	4
Cabinet-maker	4	Shoe and Boot-maker or mender	91
Wheelwright	29	Shopkeeper (Dealer in sundry necessary articles, such as are sold in a village shop)	28
Sawyer	4	Tailor, Breeches-maker	49
Carrier	6	Tanner	3
Cheesemonger	1	Whitesmith	1
Coach-owner, Driver, &c.	2		
Coal-merchant, (Fuel)	3		
Cooper	3		
Currier	1		
Earthenware, China, Pottery	2		
		(33 Trades)	454

[*To be continued.*]

Professor Leybourn's *Mathematical Repository*. No. XXIII.

The Number now before us of this valuable work contains, Mathematical Notices.—Solutions to the Questions proposed in No. XXI. by several Contributors.—Constructions and Expressions for the Circumference of a Circle.—On Spherical Geometry, by T. S. Davies, Esq. F.R.S.E. F.R.A.S.—Propositions in the Lunar Theory, by the Rev. Brice Bronwin.—On the Attraction of an Oblate Spheroid of Small Ellipticity, and a Dissertation on the Expansion of Functions; by the same.—Four Indeterminate Problems, by Mr. John Davy.—Solution to a Physical Question, by Mr. Woolhouse.—On the Cycloid, by Lieut. Colonel Glover.—Properties of Plane Triangles, and Two Indeterminate Problems, by James Cunliffe, Esq.—The Rev. Charles Wildbore's Demonstrations of the Rev. John Lawson's Sixty Theorems.—On the Theory of Elliptic Transcendents, by James Ivory, Esq. A.M. F.R.S.—Dr. Matthew Stewart's Generalization of the Fourth Proposition of the Fourth Book of Pappus; together with other Theorems.—Cambridge Problems: and twenty new theorems for solution in a future Number of the Repository.

XLV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

1832. Feb. 29.—A paper "On the Secondary Formations in the neighbourhood of Ludlow," by J. R. Wright,

Esq., employed on the Ordnance Trigonometrical Survey, and communicated by Col. Colby, F.G.S., F.R.S., &c., was first read.

The district described in this memoir occupies a surface of about 167 square miles around Ludlow, and consists of clay-slate, transition limestone, with accompanying beds of shale, old red sandstone, carboniferous limestone, the coal measures, and basalt.

A letter from Sir John Herschel, K.C.H. to Roderick Impey Murchison, Esq., P.G.S., "On the Cause of the Subterranean Sounds heard at Nakoos, near Tor in Arabia," was then read.

The remarks of the author relate to a communication by Mr. Greg, which was read before the Society on the 27th of April 1831. He suggests, as the only probable explanation which occurs to him, that the phænomena may be owing to a subterraneous production of steam, by the generation and condensation of which, under certain circumstances, sounds are well known to be produced. They belong to the same class of phænomena as the combustion of a jet of hydrogen gas in glass tubes.

The author makes the general remark, that wherever extensive subterraneous caverns exist, communicating with each other or with the atmosphere by means of small orifices, considerable difference of temperature may occasion currents of air to pass through those apertures with sufficient velocity for producing sonorous vibrations. The sounds described by Humboldt, as heard at sunrise by those who sleep on certain granitic rocks on the banks of the Orinoco, may be explained on this principle.

The sounds produced at sunrise by the statue of Memnon, and the twang, like the breaking of a string, heard by the French naturalists to proceed from a granite mountain at Carnac, are viewed by the author as referrible to a different cause, viz.: to pyrometric expansions and contractions of the heterogeneous material of which the statue and mountain consist. Similar sounds, and from the same cause, are emitted, when heat is applied to any connected mass of machinery; and the snapping often heard in the bars of a grate affords a familiar example of this phænomenon.

March 14.—A paper was read, which described,

1st, The structure of the Cotteswold Hills and country around Cheltenham:

2nd, The occurrence of stems of fossil plants in vertical positions in the sandstone of the inferior oolite of the Cleveland Hills; By Roderick Impey Murchison, Esq., P.G.S. F.R.S., &c.

I. *Structure of the Cotteswold Hills and district around Cheltenham.*

The formations constituting the Cotteswold Hills and Vale of Gloucester, in the neighbourhood of Cheltenham, are described in the following descending order.

(1.) Forest Marble, the upper members of which consist of clays, containing slaty beds, the equivalents of the Stonesfield slate (Sevenhampton Common, &c. &c.). The lowest member of this group is a hard calcareous grit, which caps the hills of Lineover and Leckhampton, and is peculiarly distinguished by the abundance of a Gryphæa,

a variety of *G. cymbium?* together with *Lima proboscidea, Pholadomya ambigua* and *P. fidicula, Trigonia striata*, &c. &c.

(2.) Great Oolite—consisting of upper and lower rags, inclosing a fine-grained building-stone, the united thickness of which in the precipitous escarpment of Leckhampton is estimated at upwards of 120 feet. The fossils are nearly the same as those of the great oolite of Bath. The Bradford clay and Fuller's earth are entirely absent, the upper rags of the great oolite being separated from the forest marble by only a small loamy wayboard of a few inches, and the lower rags pass into the inferior oolite.

(3.) The Inferior Oolite is described at its maximum thickness in Crickley Hill, occupying about 60 feet, whence it thins off in its range to the north-east, presenting about half that thickness beneath Cleeve Clouds. In this district the formation assumes a remarkable mineral aspect; for, although it contains some subordinate beds of oolitic structure, it is in general made up of coarse concretions, which, being flattened, give to it the appearance of a nummulite rock. Numerous coralline bodies are described as being spread over the sandy, ferruginous faces of the stronger beds. Among the fossils there are many species common to other formations of the oolitic series.

(4.) The Lias formation having usually a cap of marlstone, the upper lias shale of Yorkshire being wanting, is observed to rise to heights ranging from 300 to 500 feet above the Vale of Gloucester, beneath which it has been penetrated at Cheltenham to the depth of 230 feet; so that the greatest thickness of the formation is estimated at about 700 feet.

The marlstone is best seen in the insulated hills of Robinswood and Church Down, in the first of which the principal stratum is a thick-bedded, calcareous grit, separated from a covering of sandy and ferruginous, inferior oolite by thin courses of marl and marlstone.

On Church Down, of which it constitutes the summit, the marlstone is quarried to the depth of 16 or 20 feet, in beds of hard, blue and grey calc-grit, abounding in *Gryphæa gigantea* and *Belemnites pencillatus*. In the Cotteswolds, this subformation has been detected by the author in the form of only a finely laminated, micaceous sandstone, alternating with marls, on which the springs generally burst forth after percolating through the strata of the inferior oolite —thus giving rise to the Chelt and other tributaries of the Severn, as well as to the Isis or Thames.

The upper beds of the lias, beneath the marlstone, are best exposed near the culminating part of the new London and Cheltenham road, which traverses the Cotteswolds at their lowest point, viz., about 500 feet above the sea, and where a great denudation of the overlying oolites has taken place. Here, these beds are rich in fossils, including *Ammonites Walcottii, A. undulatus*, Nucula (nov. spec.), *Inoceramus dubius, Belemnites acutus, B. tubularis*, and *B. pencillatus*, &c. &c.

Below this point the sloping sides of the escarpments are obscured by accumulations of the detritus of the superior formations, and the

same accumulations extend in the form of gravel and sand over a great portion of the low country around Cheltenham, the lias protruding in small knolls. At Cheltenham the superficial beds of the lias marls are loaded with the *Gryphæa incurva*, *Ammonites subarmatus?*, and a small species of Ammonites, which is very abundant, the strata being highly pyritous. Towards the base of the formation, thin bands of compact lias limestone occur; and at Comb Hill, 5 miles N.W. of Cheltenham, these dark coloured hard bands are underlaid by thick beds of white lias inclosed in thinly foliated, black shales, which are seen to be incumbent on the green and red marl of the new red sandstone, the whole dipping to the S.E.

(5.) New Red Sandstone. The author describes merely the hard green and red marl, or upper member of this formation, which is in immediate contact with the lias, on the left bank of the Severn.

Dislocations in the Cotteswold Hills.—Remarkable instances of disruption are exhibited in many upland coombs and valleys, where the marlstone or surface of the lias is laid bare, and the strata of the great and inferior oolites, on opposite sides of such depressions, dip in different directions and at high angles, frequently inclining inwards or below the superior masses of the hills. Seeing that the overlying slaty beds of the forest marble usually maintain their horizontality, and that the above derangements are partial, the author refers them to local subsidences, which may in many cases have been in great measure occasioned by the undermining effects of springs, acting upon the pyritiferous and decomposing beds of the lias.

Mineral Waters of Cheltenham.—The upper strata of water in the lias of Cheltenham containing 27 parts of chloride of sodium, and $17\frac{1}{2}$ of sulphate of soda; whilst the water obtained by the deepest sinkings contains $72\frac{1}{2}$ parts of the chloride of sodium, and only $6\frac{3}{4}$ of the sulphate of soda; the author was led to believe that the true source of the sea salt in these waters is the new red sandstone. He was confirmed in this conjecture by observing that the mineral waters occurring along the edge of the escarpment where the lias is very thin and directly incumbent on the red marl, are almost pure brine springs (Gloucester, Tewkesbury, &c.). By the dip of the strata to the S.E. these salt waters must necessarily be carried to considerable depths below the town of Cheltenham; and he conceives that they are raised to their original levels by cracks and fissures, and passing through certain soft and pyritous beds of the lias, obtain their peculiar medicinal properties. Geological evidence is thus brought in support of the views of Dr. Daubeny, which explain under similar circumstances the chemical changes of muriated into sulphated waters.—See *Phil. Trans.* 1830.

II. *On the occurrence of stems of fossil plants in vertical positions in the sandstone of the inferior oolite of the Cleveland Hills.*

After a short illustration of the nature and arrangement of the different members of the oolitic series in the north of Yorkshire, for fuller details of which he refers to Phillips's Geology of Yorkshire, and having mentioned a vast number of new species of fossils, collected on

the coast of Scarborough by Messrs. Bean, Dunn, and Williamson, the author proceeds to give a particular account of a discovery recently made by himself of the stems of Equisetum columnare, arranged in vertical positions in the escarpment of the lower carboniferous sandstone of the oolite at Carlton Bank, near Stokesley, Yorkshire. A similar phænomenon was first made known by Messrs. Young and Bird, and subsequently by Mr. Phillips, as respected a portion of the coast between Scarborough and Whitby; but owing to the limited field in which it was observed, nearly all geologists continued to be of opinion that the plants thus found had been accidentally collocated by drifts and currents of water. The recent discovery of these stems in an upright position in the same stratum, far in the interior, and 40 miles distant from that point of the coast where they were first noticed, induced the author of this memoir to infer that this peculiar arrangement, at points so distant from each other, could not have been fortuitously produced, and that therefore these plants like those of the dirt bed in Portland*, are still in the place of their growth. The author had observed the vertical stems in the Yorkshire coast in the year 1826; and in returning to Scarborough last summer, after making the discovery at Carlton Bank, he was confirmed in the conclusion to which he had arrived, by learning from Messrs. Williamson and Bean that all the Equiseta found by them in the lower sandstone and shale, since his first visit, were invariably in vertical positions. He further ascertained that the only fossil shell which had been detected in the associated strata of the lower sandstone and coal, was a fresh-water bivalve; and the fine lamination of the beds indicated that they must have been formed in a tranquil manner. In the overlying formations, on the contrary, all the fossil shells are of marine origin; and although in one of them vegetable matter and coal are also found, yet the stems of the Equisetum are never vertically arranged as in the lower sandstone, but are confusedly mixed up with other vegetable detritus.

From these data the author concludes, that during the formation of the sandy *lower* oolite of Yorkshire, the dark, shale beds in which the Equiseta still seem to be rooted, were exposed to the atmosphere— that these stems have never been detached from the place of their growth, but have been sustained in their original positions, having been first gradually silted up, and then buried under the accumulations of an estuary, the matter in which having consolidated round them, has retained the forms of their lower parts;—that afterwards these vegetable and carbonaceous strata were covered by a sea in which the shells of the middle oolite were deposited, and into which the rolled plants found in the *upper* sandstone and shale were transported.

March 28.—A paper was first read, entitled "A Sketch of the Geology of Pulo Pinang and the neighbouring islands," by J. W. Ward, M.D., Assistant Surgeon of the Madras Establishment, and communicated by the President.

* See the abstract of Dr. Buckland and Mr. De la Beche's paper on Weymouth, Phil. Mag. and Annals, N. S. vol. vii. p. 455.

Pulo Pinang, or Prince of Wales's Island, is stated to consist of a central mountain range, with plains on the eastern and western sides. The mountains are said to be composed wholly of granite, varying in the size and the proportion of the constituent mineral; and to be traversed by veins of quartz and finely grained granite. The plains are described as formed entirely of alluvial matter, in which no animal remains have been found. The author conceives that these plains have been gained from the sea, which, according to his opinion, once washed the foot of the mountains. Stream tin, in small quantities, is stated to occur near Amees Mills, but no veins of this mineral have been found. The sea is said to be making considerable ravages on some parts of the coast, but on others to be depositing extensive mud banks. Of the neighbouring islands Pulo Rimau, Pulo Jerajah, Pulo Ticoose, and Pigeon Island, consist of granite; Pulo Boonting, of felspathic rocks; Pulo Sonsong, the Pulo Kras, Pulo Kundit, of argillaceous schists; Pulo Bidan, of limestone resting on argillaceous schist; and Pulo Panghil, of limestone similar to that of the island last mentioned.

A paper was then read, entitled, "An attempt to bring under general geological laws the relative position of metalliferous deposits, with regard to the rock formations of which the crust of the earth is formed," by M. Albert Louis Necker, For. Mem. G. S. &c.

The author commences by remarking, that ancient writers failed in their attempts to establish fixed rules for recognizing metalliferous districts by the external configuration of the soil; and that the laws which guide the miner in discovering new metalliferous veins in one country will often not assist him in another. He next observes that, as far as he is aware, Werner and his disciples abandoned the idea of establishing a connexion between formations and metalliferous deposits; and that Hutton considered the connexion of veins and the rocks through which they pass to be purely fortuitous. He then states, that he believes Dr. Boué* was the first to point out, in a general manner, the relative position of metalliferous veins and primary unstratified formations; and thus to lead to the inference, that the metals were deposited in the former by sublimation from the latter: and he adds, that Baron Humboldt† accounts for the association of the mines of the Oural and Altai mountains with granite, porphyry, and syenite, by supposing all of them to be the effect of volcanic agency, taken in its most extended signification.

This doctrine, the sublimation of the metalliferous contents of veins from igneous matter, the author states, occurred to him twelve years ago, from observing the deposition of specular iron on the crust of a stream of lava flowing down the side of Vesuvius; and he was induced from that circumstance to institute a series of inquiries, and in further prosecution of the subject, he proposes in the memoir the following questions:—

1st, Is there near each of the known metalliferous deposits any unstratified rock?

* *Mémoire Geologique sur l'Allemagne.*

† *Essai de Géologie et de Climatologie Asiatique.*

2ndly, If none is to be found in the immediate vicinity of such deposits, is there no evidence, derived from the geological constitution of the district, which would lead to the belief that an unstratified rock may extend under the metalliferous district, and at no great distance from the surface of the country?

3rdly, Do there exist metalliferous deposits entirely disconnected from unstratified rocks?

With respect to the first of these questions, the author shows, by copious references to works on England, Scotland, Ireland, Norway, France, Germany, Hungary, the southern Alps, Russia, and the northern shores of the Black Sea, that the great mining districts of all these countries are immediately connected with unstratified rocks: and in further support of this solution of the first question, he mentions the metalliferous porphyries of Mexico, and the auriferous granite of the Orinoco; but he observes that his knowledge of the mining countries of South America is not sufficient to enable him to state their general geological connexions.

With reference to the second question,—the probable association of metallic veins with unstratified rocks, though the latter are not visible in the immediate neighbourhood of the former;—the author gives a section of the country between Valorsine and Servoz, and points out the probable extension of the granite of Valorsine under the Aiguelles Rouges and Breven, composed of protogine, chlorite, and talcose schists, to the immediate vicinity of the mines of Servoz, which are situated in the latter formation. He also refers the reader for further illustration to the metallic deposits of Wanlockhead and the Lead-hills; to the mines of Huelgoet and Poullavaen in Brittany; to those of Macagnaga and Allayna at the foot of Mount Rosa, of Cardinia, Corsica, and Elba; to the metalliferous veins of the Vosges, Brescina in the Alps, and the Altai chain;—all of which occur in districts where unstratified rocks are known to exist.

The author, however, states that besides the evidence thus afforded of the connexion of igneous rocks with metalliferous deposits, it is necessary to have a knowledge of the stratification of the formations in which mines are worked before any legitimate conclusion can be drawn.

In reply to the third question,—Do there exist metalliferous deposits entirely disconnected from unstratified rocks?—The author enumerates the mines of the Netherlands; those of quicksilver at Idria; the lead mines of Poggau in the valley of the Mur; Pezay and Macoz in the Tarentaise; and the veins of galena in the mountain-limestone of the south-west of England.

The author then gives, as a general illustration of his subject, a sketch of the countries between the Alps and the western extremity of England, and shows that igneous rocks and metallic deposits are totally wanting in the whole of the districts extending from the foot of the Alps across the valley of Lac Leman, the Jura chain, the plains of Franche Comte and Burgundy; and in the oolitic, green-sand, chalk and tertiary formations of the north-west of France, and in the tertiary and secondary formations of England as far as Devonshire;

but that, on the contrary, as soon as the unstratified rocks recommence in the last-mentioned district, metallic veins reappear.

Lastly, the author compares the relative connexion of igneous deposits with metallic accumulations, and states that ores are more abundant in granite, certain porphyries, syenites, amygdaloids, and trap, which he calls underlying, unstratified rocks, than in the newer porphyries, the dolorites, and the true volcanic formations, which he distinguishes by the term of overlying, unstratified rocks; and he alludes to the assistance which the practical miner would derive from attending to this distinction, and to the principal object of the paper, —the connexion of igneous with metalliferous deposits.

April 11th.—A Letter from George Gordon, Esq., addressed to Roderick Impey Murchison, Esq. P.G.S., noticing the existence of lias on the southern side of the Murray Firth, was first read.

Mr. Gordon, after referring to the memoir of Professor Sedgwick and Mr. Murchison on the North of Scotland, in which lias is shown to occur on the northern side of the Murray Firth, points out the existence at Linksfield or Cutley-hill near Elgin, of a stratum of clay inclosing thin bands of limestone, and occupying a position analogous to that of the lias on the northern side of the Firth. Mr. Gordon likewise states, that in making the canal to drain Loch Spyine, a bed of clay was penetrated containing numerous specimens of Belemnites; and he conceives that a great part of the bay of Lossiemouth belongs to that formation.

A paper was then read "On the strata in the immediate neighbourhood of Lisbon and Oporto," by Daniel Sharpe, Esq. F.G.S. & F.L.S.

Lisbon is shown, by the author of this memoir, to stand upon a range of hills divided by a narrow valley or ravine. The eastern division of the range is stated to be composed of tertiary deposits, and the western of a limestone containing Belemnites, both which are described in the paper.

The next formation, in a descending order, is a deposit of sand and sandstone, in which no organic remains were noticed. It appears to the north and east of Lisbon, and at Villa Franca, where it underlies the belemnitic limestone. The celebrated springs of Caldas burst forth in this formation.

Beneath the sandstone last mentioned, the author observed at Villa Nova da Reinha, to the north of Lisbon, another bed of limestone; but he gives no details respecting its nature.

The next formation described in the memoir is an extensive deposit of basalt, which is stated to occur in contact both with the tertiary series and the belemnitic limestone, but to have produced no change on these strata at its junction with them.

The granite of the hill of Cintra is said to be composed principally of quartz and felspar with a small proportion of mica and hornblende, and to be divided into large blocks by natural lines of cleavage. On the north side of the hill a limestone is stated to rest against the granite, and on the east a deposit of shale, and the strata of these formations to be highly inclined.

The author next proceeds to describe the structure of the neighbourhood of Oporto. The city stands upon a low ridge of granite, cut through by a defile in which the Douro flows. The granite, composed of quartz, felspar, mica, and hornblende, in the immediate vicinity of Oporto is hard, but at a short distance from it, is decomposed even to a considerable depth beneath the surface.

To this formation succeed, a granitic gneiss, chlorite slate, alternate beds of anthracite and conglomerate derived from the subjacent rocks, and chlorite slate again.

An Essay "On the Curvilinear Structure of Lava," by Signor Monticelli of Naples, was afterwards read.

The object of the author is to attract the notice of geologists to a peculiarly beautiful and symmetric arrangement which he has observed in the lava of La Scala, one of the largest and most ancient currents of Vesuvius. The existence of numerous perpendicular and horizontal fissures which traverse this lava, and sometimes give it the appearance of regular stratification, was described by Breislac; and the same observer noticed its tendency to split, under the hammer, into irregular prisms of an hexagonal figure. But a far more symmetric arrangement was recently discovered in a grotto opened by the workmen in quarrying the lava. The walls of lava bounding this grotto were distinctly curvilinear; several distinct curvilinear strata were traced with their seams parallel to each other; and the grotto itself, decreasing in height and width towards either extremity, possessed the form of an ellipsoid. The author describes another similar arrangement of the lava at the same locality, consisting of not fewer than fourteen successive, parallel strata of a spherical form, arranged one above the other in such a manner as to present the outline of an inverted, truncated cone.

The author, after referring to similar though less perfectly developed curvilinear arrangements which have been seen in lava and basalt in other situations, throws out suggestions as to the cause of these remarkable appearances. He objects to the opinion of Breislac, that the vertical and horizontal fissures noticed by him are referrible to contraction produced by the sudden cooling of a heated mass; and he adduces an instance of a lava current having flowed into the sea, and been thereby subject to most rapid refrigeration, without possessing the least fissure in its substance. The author believes that the production of fissures, of prismatic forms, and of the curvilinear arrangements, in lava and basalt, depends on uniform forces of attraction acting on the mass while in a fluid condition. He appeals, in particular, to the spherical, elliptic, and parabolic forms observed by himself in proof of the agency of central points of attraction having acted on surrounding particles, and influenced their arrangements.

May 2.—A paper was read, "On the Geological Structure of the North-eastern Part of the County of Antrim," by James Bryce, Jun. Esq. M.A. Member of the Belfast Natural History Society, &c., and communicated by Roderick Impey Murchison, Esq. P.G.S.

In this memoir the author enters into a minute description of the physical features and geological constitution of a portion of the di-

strict, described by Dr. Berger, and by Dr. Buckland and Mr. Conybeare in the third volume of the first series of the Geological Society's Transactions.

After alluding to the labours of these celebrated observers, the author defines the extent and physical features of the district described in his memoir. He states that it is bounded on the west by the escarpment of the chalk from Kenbaan Head to Corky; on the south by a line drawn from that place to Gerron Point; and on the east and north by the Irish Sea. The area, thus circumscribed, is traversed in a N.W. direction by the Aura mountains, from the southern part of which several, long, projecting ridges with flat, broad summits and precipitous sides, branch off; and in the northern part of the district the surface is occupied by detached hills, having a direction parallel to that of the main chain. The height of the principal mountains varies from one thousand to two thousand feet. Their eastern declivity is abrupt, but their western is formed by a succession of undulating hills, which gradually descend into the low country extending from Kenbaan Head to Corky.

The principal formations described, are mica-slate, porphyry, old red sandstone, carboniferous limestone, coal measures, new red sandstone and conglomerate, lias, mulatto or green sand, chalk, and trap.

May 16.—A paper "On the Geological Relations of the stratified and unstratified Groups of Rocks composing the Cumbrian Mountains," by the Rev. Adam Sedgwick, V.P.G.S. F.R.S. Woodwardian Professor in the University of Cambridge, was read.

Chap. I.—*Introduction.*

The author first shows, that the limits of the region to be described, are defined by a zone of carboniferous limestone, based here and there upon masses of old red conglomerate. This zone is described as entirely unconformable to the central system, and for the phænomena presented at the junction of the two great classes of rocks, he refers to previous memoirs read before the Society.

The rocks of the central system are separated into stratified and unstratified; and the stratified are divided into four distinct groups, in the following descending order:

1. Greywacke and greywacke-slate; the whole group based on beds of limestone and calcareous slate, and bounded at its upper surface by a part of the carboniferous zone.

2. A great formation of quartzose, chloritic, roofing slate and felspar porphyry; alternating in great, irregular, tabular masses, each passing into, or replacing, the other; the whole having nearly a constant strike, and dip similar to that of the preceding group.

3. Skiddaw slate—a very fine, dark, glossy clay-slate, occasionally penetrated by quartz veins, sometimes passing into a coarse greywacke and greywacke-slate.

4. Crystalline slates between the preceding group and the central granite of Skiddaw Forest.

It is then shown, that the mineralogical axis of the whole region may be placed in the direction of a line drawn from the centre of Skdidaw Forest to Egremont, and that on the north side of this line

the second group reappears immediately under the carboniferous zone, forming a band which gradually thins off, and disappears below Cockermouth.

The unstratified groups are then enumerated as follows:

1. Granite of Skiddaw Forest, the true mineralogical centre of the whole region.

2. Carrock Fell syenite, irregularly traversing and overlying the third and fourth stratified groups, and apparently underlying the second.

3. A great formation on the S.W. side of Cumberland, composed of syenite, porphyry, and granite, which breaks through between the second and third groups, penetrating, traversing, and overlying the third, but never overlying the second.

4. Shap granite, breaking through, between the first and second great, slaty groups, and cutting off the range of the fossiliferous limestone by which they are separated from each other.

5. Granite veins; porphyritic dykes, having the relations of the Cornish elvans; common trap dykes: these are found associated with all the stratified groups.

Chap. II.—*Successive stratified groups.*

§ 1. *Greywacke and greywacke-slate.*—This group is subdivided as follows, in descending order:

1. Coarse greywacke and greywacke-slate, occasionally with organic remains, but with no beds of limestone.

2. Finer greywacke-slate, thrown into great undulations, but having a prevailing strike about N.E. by E.

3. A band of calcareous slate and fossiliferous limestone, ranging from the hills north of Dalton to Coniston-water-foot.

4. A broad zone of greywacke-slate, having generally a strike about N.E. by E. and a dip S.E. by S. at an angle varying from 30° to 45°. From this zone masses of roofing slate are commonly derived by a cleavage transverse to the plane of stratification.

5. Calcareous slate and limestone, ranging from the south-western extremity of Cumberland till it is cut off by the Shap granite. Its range, and the evidence it offers of great dislocations, have been described in a previous memoir (see Phil. Mag. and Annals, vol. ix. p. 211, 377).

§ 2. *Green slate and porphyry, &c.*—This great group, which occupies all the highest and most rugged mountains of the region described in this memoir, is essentially composed of great, tabular masses (having generally the same strike and dip as the lower beds of the preceding group), composed of different modifications of porphyritic and felspathic rocks, and of quartzose and chloritic slate, all the finer portions being derived from a cleavage transverse to the stratification of the beds. The modifications of the slate are first described, and it is shown that they pass, on one hand, into compact felspathic slate sometimes porphyritic; on the other, into coarse granular and concretionary slaty masses, and through them into breccias, or pseudo-breccias, all these changes being effected without any change of strike or dip. In like manner it is shown that the amorphous, and even semicolumnar, prismatic, porphyries are not only

arranged in directions parallel to the tabular masses of green roofing slate; but pass themselves into a slaty texture with a strike and dip parallel to those of the true roofing slate. They also pass into brecciated masses similar to those which form a part of the slate groups. From these facts,—as well as from the negative facts, that the porphyries never penetrate the roofing slate in the form of dykes, and produce no mineral change in the limestone beds resting on them,—it is inferred that the whole group is of one formation, which has originated in the simultaneous action of aqueous and igneous causes long continued.

§ 3. *Skiddaw slate.*—The author briefly describes the range and extent of this group, its position below the preceding, and some of its mineral changes from fine, glossy, clay-slate, much penetrated by quartz veins, into, though rarely, very coarse greywacke. It does not generally effervesce with acids, and contains no organic remains: it is chiefly distinguished from the first group above described, by these negative properties, and by its being of finer texture.

§ 4.—*Crystalline slaty rocks* in the central portions of Skiddaw Forest, *immediately* between the preceding group and the central granite.

This group is described as being irregular in its order and ill exposed, but from the comparison of a series of sections appears to be separable into the following subdivisions.

(1.) Skiddaw slate with interspersed crystals of chiastolite, alternating with and passing into the preceding group.

(2.) A similar slate with numerous crystals of chiastolite, passing in the descending order into a crystalline slate sometimes almost composed of matted crystals of chiastolite.

(3.) Mica slate spotted with chiastolite.

(4.) Quartzose and micaceous slates sometimes passing into the character of gneiss.

With this group the paper terminates: but the author promises to resume the subject, and describe, in order, first the several unstratified masses above enumerated; and then the changes produced by the protrusion of the unstratified masses, both on the position and mineral character of the several stratified groups.

May 30th.—A paper was first read "On the Basalt of the Titterstone Clee Hill, Shropshire," being the concluding part of a memoir on the Ludlow district, laid before the Society on the 29th of February, by J. Robinson Wright, Esq., F.G.S. employed on the Ordnance Trigonometrical Survey (see p. 221).

The basalt occupies the two highest points of the hill, called the Giant's Chair and the Hoar Edge, which are separated from each other by a narrow ravine. It rests partly upon the old red sandstone, and partly upon the coal measures; and occasionally assumes a columnar structure,—the prisms inclining at an angle of 75°. Besides these overlying masses a basaltic dyke has been ascertained to cut through and greatly affect the coal measures; and the author suggests that the outburst of this dyke may, from its direction, possibly form the north-westerly escarpment of the Hoar Edge.

The author, in conclusion, compares the Titterstone basalt with the trap of Rowley Regis, and points out their agreement in geological position and mineralogical structure.

A paper was then read "On a large Boulder-stone on the Shore of Appin, Argyleshire," by James Maxwell, Esq., and communicated by William Smith, Esq., F.G.S., F.R.S., &c. &c.

This boulder-stone consists of a granitic compound of quartz, felspar, and mica; the last mineral being the principal ingredient. Its form is irregular, but the angles have been rounded. The greatest vertical circumference is forty-two feet, and the greatest horizontal thirty-eight feet. It is supported on three smaller stones, each about six inches thick; one of them being a granite of a paler colour than that which composes the boulder itself; and the other two consisting of argillaceous ironstone. The formation on which the supports rest is a slaty, calcareous sandstone. Numerous other granitic boulders occur in this part of Scotland, but no rock *in situ* from which they could have been derived.

A third paper was read "On the Discovery of Bones of a Rhinoceros and a Hyæna in one of the Cefn Caves, situated in the Vale of Cyffredan, Denbighshire," by the Rev. Edward Stanley, F.G.S., &c.

The author commences his memoir by describing the physical features of the district, and the present mode by which its waters are drained. He then shows that if the pass between the Cefn and Galltfaen cliffs were filled up, the river Elwy would be converted into an extensive lake which would occupy the vale of Cyffredan, on the eastern side of which the Cefn caves are situated. The lowest cave, raised but a few feet along the level of the river, forms a natural archway penetrating through the limestone cliff and affording a passage for a road. In its lateral ramifications, human bones, the horns of a deer, and works of art have been found, but no remains of extinct animals. About one hundred feet above the level of the valley, two other caves are situated in the face of the precipitous, limestone cliff; but only one of them has been examined, and it is to this cave that the memoir in particular refers. When it was first discovered, the interior, from the level of the entrance to a short distance from the roof, was occupied by calcareous loam, in which a few angular masses of limestone, part of the humerus of a rhinoceros, teeth of a hyæna, and numerous fragments of bones were found. Beneath this accumulation, and beneath what had been considered the floor of the cave, the author ascertained the existence of another deposit of similar loam; but containing, besides, fragments of bones and small portions of wood, rounded pebbles of greywacke.

The cave was found to have several branches, one of which was traced, in a southern direction, through the hill till it terminated in the face of the escarpment opposite the Galltfaen cliff; but the real extent of the other branches has not been determined.

The author, after these details, enters into an inquiry respecting the former physical structure of the district, and the mode by which the contents of the cave were deposited in the position in which they were found. He conceives that either the vale of Cyffredan was

formerly occupied by a lake, or that the surface of the vale was once nearly on a level with the entrance of the cave: and he explains the position of the loam and associated pebbles and bones by supposing that a sudden flood, rushing through the valley, carried into the cave the pebbles, fragments of wood, and loam found in its lower part:—that after this inundation no similar catastrophe occurred for an unknown period, during which the caves again became the resort of wild animals:—that at the close of that period another and more powerful flood occurred, rising above the level of the caves, and depositing within it, the loam which occupied the greater part of its cavity; and that this flood, overcoming every obstacle, excavated the valley to its present depth.

The memoir was illustrated by numerous drawings of the caves and bones, a ground plan, and a manuscript map of the district.

June 13.—A paper was first read, entitled "Observations on the London Clay of the Highgate Archway," by Nathaniel Wetherell, Esq., F.G.S.

This communication, which was accompanied by a series of specimens, gives a full account of the position, extent, and order of the beds cut through in making the excavation for the archway, and a list of the fossils found in the lowermost stratum or the London clay.

For the details respecting the order of the beds the author refers to the "Outlines" of the Rev. William Conybeare and the late Mr. Phillips; and after enumerating the fossils found in the clay, points out that the species of most common occurrence were *Pectunculus decussatus, Natica glaucinoides, Modiola elegans,* and *Teredo antenauta,* and that those of rarest occurrence were *Acteon elongatus, Cypræa oviformis, Neritina concava,* and *Serpula crassa.*

A paper was afterwards read giving "An account of the Discovery of portions of three Skeletons of the Megatherium in the province of Buenos Ayres in South America," by Woodbine Parish, jun. Esq., His Majesty's Chargé d'Affaires and Consul General at Buenos Ayres; followed by a description of the bones by William Clift, Esq. F.G.S. F.R.S. &c. &c.

Mr. Parish some years since presented to the Geological Society several large bones of mammalia, discovered in the valley of Tarija on the confines of Bolivia, and being anxious to procure further specimens, he instituted a series of inquiries, by which he ascertained that the teeth and bones of quadrupeds had been frequently met with in the province of Buenos Ayres, especially in the neighbourhood of the river Salado, and in the beds of its tributary lakes and streams; as well as in the adjoining province of Entre Rios, and that in the Banda Oriental a nearly perfect skeleton was once found.

During these inquiries Mr. Parish was informed that some bones of extraordinary size had been found in the bed of the Rio Salado, and brought to Buenos Ayres from the Estancia of Don Hilario Sosa. On inspecting them he was immediately struck with their resemblance to the remains of the Megatherium formerly sent to the Museum at Madrid by the Marquis of Loreto, and likewise procured in the province of Buenos Ayres. These bones, the property of Don Hilario

Sosa, consisted of a pelvis, nearly perfect, a thigh bone, several virtebræ, five or six ribs, and four teeth. After much solicitation Mr. Parish became possessed of them, and in the hopes of procuring the remainder of the skeleton, he deputed Mr. Oakley, a gentleman of the United States, to make the necessary investigations.

Mr. Oakley soon ascertained that other bones were imbedded in the mud at the bottom of the river, and by diverting, in part, the course of the stream, he succeeded in obtaining a scapula, an os femoris, five cervical vertebræ, several teeth, and numerous other bones which were too much decayed to be preserved.

Besides these valuable remains Mr. Oakley procured parts of two other skeletons of the Megatherium; one of them from a small rivulet near Villanuéva, and the other from the banks of the lake at Las Aveiras. Both these skeletons were accompanied by a thick osseous covering, or shell, considerable portions of which were preserved, and form part of the collection sent to England by Mr. Parish.

The preceding history of the discovery of the bones of the Megatherium, was succeeded by an enumeration and description of them, by Mr. Clift; from which it appears that the parts of the skeleton brought to England by Mr. Parish, although comparatively much less numerous and complete than those in the specimen preserved in the Royal Cabinet at Madrid, fortunately include several essential parts which are deficient in that specimen; and that consequently from the discovery of these remains, the history of the animal will be much improved. Of the hitherto undescribed parts, the structure of the teeth,—the existence of the pubis and ischium,—and a large proportion of the caudal vertebræ, are the most important and essential additions to our previous knowledge of this most singular and stupendous creature.

ROYAL ASTRONOMICAL SOCIETY.

Feb. 10.—*Extracts from the Report of the Council of the Society to the Twelfth Annual General Meeting, held this day.*

In compliance with the Bye-laws, the Council now report to the members at large, the progress and state of the Society, at this their twelfth anniversary. During the past year, an important alteration in the condition of our body, alluded to in the last Report, has taken place: a Royal Charter has been obtained; and, conformably thereto, an altered and amended set of Bye-laws has been agreed to by the Society, differing however but little from those which were before in existence; the principal alterations being such as were required by this new state of things.

Amongst the deaths which have occurred in the preceding year, the Council regret the loss of two very valuable members of the Society, as well as two of its Associates: viz. the Rev. Fearon Fallows, late astronomer at the Cape of Good Hope, Capt. Foster of the Royal Navy, M. Pons of Marlia, and the Abbé Grégoire.

Mr. Fallows is an example, and, in this country, happily, not a

solitary example, of the influence which talents and character may have on the fortunes of an individual, under circumstances apparently the most untoward. He was born, July 4, 1789, at Cockermouth, in the county of Cumberland, and his early years were spent in following his father's occupation, that of a weaver, with no further time or opportunity for education than could be afforded by the ordinary intervals of labour. Fortunately, his father was himself a man of considerable information and studious habits, and devoted these leisure moments to the education of his child, who thus became early acquainted with the principles of arithmetic and geometry, subjects in which he chiefly delighted. When a mere boy, a mathematical book was his constant companion at the loom; and this taste was encouraged by the kindness of many persons in the vicinity, who supplied him with books, and such assistance in his studies as they were competent to give. His father having become parish clerk at the neighbouring church of Bridekirk, the extraordinary acquirements of the young mathematician became known to the Rev. Mr. Hervey, vicar of that parish; and by the advice and recommendation of this gentleman, Mr. Fallows was engaged as an assistant by Mr. Temple, at that time head-master of Plumbland school. On the death of Mr. Temple in 1808, Mr. Hervey further exerted himself to obtain for Mr. Fallows the patronage of some gentlemen of fortune and interest, in order that he might be enabled to go to the University. In this purpose he was successful; and in 1809, Mr. Fallows commenced residence as a student of St. John's College, Cambridge.

Whatever difficulties might have previously embarrassed Mr. Fallows' career were now dissipated. At St. John's, honourably distinguished (perhaps above all other colleges) for attention to the education and interests of unfriended merit, he found every assistance which could be desired,—kind friends, most able instructors, and an unlimited power of consulting books. His progress was, accordingly, rapid and successful, though directed, as was to be expected, in the line of the older English geometers, with whom he was already familiar, rather than according to the continental mathematicians. In 1813, he proceeded bachelor of arts, and was third, Sir John Herschel being senior wrangler.

Shortly after taking his degree, as there was no fellowship open at St. John's to which Mr. Fallows was eligible, he removed to Benet College, as mathematical lecturer; but was gladly recalled to his own college in 1815, when a fellowship became vacant. Here he resided for some years; and when His Majesty's Government had resolved upon establishing an Observatory of the highest class at the Cape of Good Hope, Mr. Fallows was selected as the person best qualified to direct the future establishment.

The few months which intervened between the time of his appointment and his removal to the Cape, were spent by Mr. Fallows in the public and private observatories of this country, in the workshops of our most celebrated artists, in the calculation of special tables, and in devising the best and simplest means of making, registering, and reducing astronomical observations.

On the 1st Jan. 1821, he married Miss Mary Anne Hervey, eldest daughter of the Rev. H. A. Hervey, vicar of Bridekirk, and embarked on the 4th of May following. Mr. Fallows arrived at the Cape on the 12th of August, 1821.

His first undertaking was an approximate catalogue of 275 principal stars, published in the Phil. Trans. 1824. From the description of the instruments employed, it will be seen that they were of a very humble description, viz., a portable transit of only twenty inches focal length, and a very indifferent altitude and azimuth instrument by Ramsden, ill-divided, and unstable in its adjustments, being indeed originally constructed as an equatorial. It is probable that the length of time which must necessarily elapse between the design and completion of a first-rate Observatory, in a foreign station, was not fully taken into account, either by the Government or the astronomer; otherwise the temporary instruments would, doubtless, have been of a very different class. The plan of the Observatory was received by Mr. Fallows in the latter part of 1825, and he immediately proceeded to carry it into effect. A site was selected about three miles from Cape Town, and Mr. Fallows lived in a tent on the spot, to determine the lines of the building, and to superintend the workmen. The foundations were dug out before the clerk of the works arrived to relieve him from this task.

In the beginning of 1829, the transit and mural circle were fixed in their places, and we might now have anticipated a season of enjoyment for the Cape astronomer; but, for some cause hitherto unexplained, the circle, to which he had looked forward with pride and exultation, proved for a long time a source of bitter uneasiness. Some part of this must, doubtless, be attributed to the shattered state of the observer's health; but the fact, that "the index error of two opposite microscopes was ever variable in different parts of the instrument, while with three microscopes, at 120° distance from each other, or with the whole six, the index error was nearly constant," was sufficiently startling to harass a person of less sanguine and zealous temper. Finally, Mr. Fallows was of opinion that some permanent injury had been received by the circle and axis, from a fall which the package received whilst it was removing from the hold of the ship at the time of landing; but that the mean of the six microscopes might be fully depended upon; since high and low stars, when observed directly and by reflexion, gave the same position of the horizontal point. Before he had come to this conclusion, which seems to have been some time in the middle of 1830, sickness deprived him of the services of his assistant, Capt. Ronald; and Mr. Fallows was left, unaided, to do the best he might with a transit and mural circle. He was relieved from this difficulty by the affection and intelligence of Mrs. Fallows, who offered to undertake the circle observations while he was engaged with the transit. A very little instruction sufficed to render her perfectly competent for this task; and the Cape astronomer had, like Hevelius, the pleasure of finding his best assistant in the partner of his affections. Some of his letters, written at this time, express a strong hope and confidence that he

should at length be able to justify the high expectations which had been formed of the Observatory, and that his work would bear a comparison in accuracy, though not in extent, with that of any other establishment.

But the labours of the Observatory were too much for a constitution already much enfeebled by previous illness. He had suffered very severely from a *coup de soleil* soon after his arrival at the Cape, while fixing the small transit; and, besides some less serious complaints, experienced a dangerous attack of scarlet fever in the summer of 1830, from which he seems never to have fully recovered. In the beginning of 1831, his health was visibly impaired, but he could not be induced to leave the Observatory before the equinox. Towards the end of March, he became incapable of struggling any longer with the disease, and went to Simon's Town; but it was now too late, and he breathed his last on the 25th July, 1831, in the forty-third year of his age.

To those who were acquainted with Mr. Fallows, it is unnecessary to dwell upon the integrity and simplicity of his character, or the depth and clearness of his understanding: as an astronomer, he had few rivals. Perfectly acquainted with the practical and scientific departments of astronomy, he carried into the Observatory the same straightforward zeal and honesty which were the distinctive features of his private character; and if his life had been spared, would unquestionably have realized the most sanguine expectations of his friends and admirers.

Mr. Fallows did not leave his observations completely prepared for publication, but so nearly as to require very little additional labour. His wish was to have had them printed under his own eye, after they had been examined and approved of by competent judges in England; for which purpose, examined copies were transmitted by him to the Lords Commissioners of the Admiralty. They consist of about 3000 transit, and several hundred circle observations, with six microscopes, and some series with the invariable pendulum. The instrumental errors are ascertained, and the current reductions computed, so that there will be no difficulty in presenting the results, though not perhaps in the independent manner proposed by the observer. It is to be hoped that these observations and reductions will be speedily published, by the order of the founders and patrons of the Cape Observatory; and we are confident, that they will be found every way worthy of Mr. Fallows and of the country which committed that important and magnificent establishment to his charge.

But though the loss inflicted upon science is thus severe, your Council are happy to state, that the Government has not at all relaxed its zeal for the Cape Observatory. An assistant, Mr. Meadows, was dispatched to aid Mr. Fallows, shortly before his decease: since that time, Mr. Thomas Henderson (a gentleman known to you all, as one of the most active and enlightened cultivators of astronomy in this country, and one to whom this Society has, upon many occasions, thankfully acknowledged its obligations) has been appointed His Majesty's Astronomer at the Cape. That this gentleman, tread-

ing in his predecessor's path, and with better health, and under better auspices, may reap the rich harvest which Mr. Fallows could only commence, is the confident wish and hope of those by whom his merit, zeal, and modesty, are appreciated.

Capt. Foster was well known to every scientific man in this country, for his active services in the expedition under Capt. Parry to the North Pole, and for his ardent zeal and great attention to accuracy in every thing which he undertook for the promotion of science. These and other excellent qualities which he possessed, led to his more immediate promotion in the Navy, gained him the reward of the Copley Medal from the Royal Society, and pointed him out as a fit and proper person to conduct a scientific expedition, at that time contemplated by the Government, towards the South; and he was soon after appointed to the command of the Chanticleer for that purpose.

The principal object of this expedition was to swing the pendulum near the equator, and also at various places in the southern hemisphere. With this view he was furnished by Government with two of Kater's invariable pendulums, No. 10 and No. 11; and also by this Society with two convertible pendulums of a new construction, one of iron and the other of copper, as described in No. 13 of the Monthly Notices, and alluded to in the Eighth Report. Capt. Foster, however, did not live to bring home the fruits of his own industry and zeal; for he was unfortunately drowned, near the close of his voyage, whilst descending the river Chagres in a canoe, towards his ship then lying at anchor.

Capt. Foster has left behind him a vast mass of important information connected with the objects of his voyage. The original copies of his pendulum experiments have been laid before the Council of this Society by the Lords Commissioners of the Admiralty, with a request that they would consider the best mode of obtaining the proper results, with a view to their being made public in the most satisfactory manner. For the attainment of this object Mr. Baily has kindly undertaken to superintend the computations, and to make such further experiments on the pendulums in London, as may be necessary to deduce the required results from the whole series of Capt. Foster's experiments. Already these supplementary experiments are completed; and the computer has also made great progress in reducing the observations from the elements furnished by Mr. Baily for that purpose: and when the whole is finished, a Report will be drawn up on the subject.

Capt. Foster's journal of his experiments is a model of his great attention to accuracy and minuteness of detail. Every necessary information is regularly entered in *printed blank forms,* with which he had been previously provided (a method which cannot be too strongly recommended in all similar cases); and there is consequently no difficulty or doubt as to the full meaning and effect of every figure that is introduced. The number of places at which Capt. Foster swung the pendulum (including London and Greenwich) is fourteen; and subjoined is a list of these places, in the order in

which they were visited, together with the number of series of experiments made at each place, and the pendulums employed at each station. It should here be, however, stated that the iron and copper convertible pendulums were each furnished with *two* knife-edges (respectively marked A and B); so that, in fact, Capt. Foster might be considered as having *six* independent and invariable pendulums, whose results might be compared with each other. The stations that are marked with an asterisk are those which were visited also by Capt. Sabine, in his voyage of experiment in the years 1822 and 1823.

No.	Stations.	No. 10.	No. 11.	Iron.		Copper.	
				A	B	A	B
1	London	8	7				
2	Greenwich..........	10	10				
3	Monte Video	25	15				
4	Staten Island	46	26	11	12	16	16
5	South Shetland	23	62	15	16	9	10
6	Cape Horn	22	27				
7	Cape of Good Hope..	25	17	28	28	23	23
8	St. Helena	21	24			9	12
9	Ascension Island ..*	39	26	10	9	14	13
10	Fernando de Noronah	21	18				
11	Maranham ....*	19	16			16	12
12	Para	18	22	12	13	14	14
13	Trinidad ....*	20	17	9	9	12	12
14	Porto Bello	21	15				
	Total number of series	318	302	85	87	113	112

The whole number of series therefore was 1017; and as each series consisted (on an average) of 10 coincidences, the total number of coincidences taken by Capt. Foster was upwards of ten thousand: thus forming a mass of observations made in various parts of the globe never before attempted by any individual, and which must have its due weight in all investigations relative to this important inquiry. In the premature death of this young and accomplished officer, the Society has to deplore the loss of a zealous and active votary to science; and his memory will be long held dear by those who were more intimately acquainted with him in the relations of private life.

M. Jean Louis Pons was for many years employed at the Observatory at Marseilles; where, though his means were extremely limited, he became universally known for his steady attention to the discovery of comets: an attention which procured him the medal of this Society. In the summer of 1819, Her Majesty Maria Louisa, of Bourbon, entered into a correspondence with Baron de Zach respecting the endowment of a first-rate observatory at Lucca; desiring him to solicit an astronomer of known eminence to preside. Three names were immediately suggested; Encke, Littrow, and Pons: and as the two former had received appointments in their own countries, the choice fell on the latter. In the mean time the Baron had repaired to Lucca, in order to select the site and direct the erection of the required edifice. It was 100 feet long, by 30 in breadth, in-

dependent of dwelling apartments; and was built on a hill in the royal park of La Marlia, four miles from the city, with an excellent command of horizon; and was munificently furnished with instruments of the best description. M. Pons was honoured with the titles of "Her Majesty's Astronomer Royal, Director of the *Astroscopic* department of the Observatory, and Emerito Professor of the Royal Lyceum." Amongst other arrangements was the payment of 100 dollars, from the queen's purse, for every comet that might be discovered: and it is remarkable that M. Pons, immediately on his arrival, detected the one forming an isosceles triangle with γ and μ *Virginis*. From such a commencement, the astronomical world had great reason to form high expectations; especially as it was decided that the observations should be published annually, after the manner of those at Greenwich. But, the energy of the institution was spent in its mere erection: it promised much, but performed nothing: and after lingering in existence about four years, it was at length formally abolished. M. Pons, after this disappointment, continued to observe with such means as he could obtain; till Leopold II. invited him to Florence, on conditions as honourable as magnificent. He accordingly went thither in July 1825, after having just recognized Encke's comet at Lucca, before his departure. The previous computation of its return had been a guide to his researches; yet it proved the excellence of his eye at the age of 64, as he saw it long before any one else.

The Abbé Grégoire, afterwards Bishop of Blois, is known to us principally as having taken an active part in the establishment of the Board of Longitude in France, in imitation of the English board, to which was to be confided the publication of the *Connaissance des Tems*. The Abbé, in common with many of his scientific countrymen, was not insensible to the great advantages to be derived, in a maritime point of view, from a well-conducted Nautical Almanac: and one of the avowed objects for establishing the board in question, was the prospect that the French might be better enabled to compete with this country, with whom they were then at war.

During the past year, the Lords Commissioners of the Admiralty have consulted the Council also on another important subject connected with the advancement of navigation. In consequence of the alterations about to be introduced into the *New* Nautical Almanac, it has been considered expedient that new *Requisite Tables* should be formed to accompany it: and, with the view of carrying this object into effect, in the most efficient and satisfactory manner, a Committee has been formed to consider, arrange, and propose such Tables as may be thought most proper for that end. This Committee has already met, and a sketch of the proposed Tables has been drawn up, but not yet agreed upon: for, in an affair of so much importance, there cannot be too much time devoted to the consideration of the subject.

The Standard scale, mentioned in the last Report, is not yet executed. The work has remained a long time stationary, waiting for the micrometer object-glasses promised by Mr. Tully: but which, up to the present time, he has not been able to complete.

No subject having been specifically brought before the Council, as

deserving of the Society's medal, on the day appointed by the bye-laws for awarding the same, it has not been adjudged this year.

Amongst the numerous works presented to the Society during the past year, the Council notice with much gratification, the Miscellaneous Works of Dr. Bradley, by the University of Oxford, edited under the able superintendence of Professor Rigaud: containing what has long been considered a desideratum in the history of Astronomy, the original observations made at Kew and at Wanstead, with the zenith sector, and the progress of his celebrated discoveries of aberration and nutation. The principal part of these valuable documents were discovered by the active and diligent search of Professor Rigaud, amongst the papers of the late Dr. Hornsby, whose family readily gave them up to the University for publication; the rest have been collected from various sources: and the volume altogether reflects great credit on the University of Oxford, and adds new lustre to the character of the distinguished astronomer whose labours it records.

The Council are happy to announce that the plan for making a minute survey of the heavens, and for the formation of some new celestial charts, under the superintendence and direction of the Royal Academy of Sciences at Berlin, appears to have been carried into full effect; and three portions of this useful and valuable undertaking now lie on the table: viz. the 10th hour in Æ by Professor Göbel of Coburg, the 14th hour by the Rev. T. J. Hussey of Chislehurst, and the 18th hour (in duplicate) by Padre Giovanni Inghirami of Florence, and M. Capocci of Naples. The respective catalogues contain a list of all the stars (reduced to the year 1800) within 15° of the equator, which are to be found in Bradley, Piazzi, Lalande's *Histoire Céleste*, and Bessel's *Zones*: whilst the charts contain, besides these stars, such other additional ones as may have been seen by the respective observers, down to the 10th magnitude exclusive. This work, when complete, will form the most extensive and accurate general catalogue of the stars that ever was produced; and will be a valuable acquisition to the practical astronomer.

To P. Inghirami the public are also indebted for another recent production, executed on the true principles of science, under the patronage of the Grand Duke Leopold II. This consists of a laborious and detailed survey of Tuscany, deduced from a base of 4488·08 toises, measured with the most rigid precaution between Pisa and Leghorn in the autumn of 1817. Besides giving an excellent series of determined latitudes and longitudes, in the course of leading his chain of triangles from the valley of the Arno to the mountains of the interior, P. Inghirami has entered into an investigation of some delicate corrections, arising from what he terms "lateral refraction;" of which an interesting memoir was communicated to the *Academica Labronica* in 1818. One great advantage attending this valuable work is the prompt and speedy manner in which it has been published; whereby the present generation, which has contributed to its production, will reap the benefit and information contained in this national undertaking: a consideration too often neglected.

The year which has just expired has also been marked by the pro-

duction of a work now on the table, too remarkable to escape our notice, whether we consider what it contains, or by whom it was written. We allude to the "Mechanism of the Heavens" by Mrs. Somerville. This lady has added to our stock the most complete account of the discoveries of continental mathematicians in physical astronomy which exists in our language. Following closely in the steps of Laplace, but diverging to take notice of the discoveries of Lagrange, Poisson, and others, she has produced a treatise, which, while it paves the way to a complete understanding of the *Mécanique Céleste,* may be substituted for it by all who do not require, we will not say the most profound, but the most widely extended knowledge of the heavenly phænomena. Commencing with a general exposition of the laws of mechanics, as in the *Mécanique Céleste,* the authoress proceeds to the proofs of the general theory of gravitation, the elliptic motions of the planets, their perturbations and secular inequalities; generally, though not universally, adopting the methods of Laplace. She then considers the lunar theory, and that of the satellites of Jupiter. At the end of each part the numerical values of the perturbations are deduced; and any particular cause, which might produce a perturbing effect, such as the resistance of a medium, or the attraction of the fixed stars, is inquired into. In a preliminary dissertation a popular account of the results is given, the whole thus forming a complete system of physical astronomy. In concluding this slight notice, your Council cannot forbear their expression of delight and admiration at the achievement of our countrywoman; or of hope, that the example thus set may stimulate others to exertion in a field which, they lament to say, has been too much neglected in England.

The vacancy occasioned by the death of Mr. Fallows, has been supplied, as already noticed, by the appointment of Mr. Henderson: and the Council cannot omit this opportunity of again* bringing before the Society the very able assistance which has been, from time to time, afforded them by the voluntary labours of this gentleman. Bound to the Society by no conventional engagement (for Mr. Henderson has but very recently become a Fellow of the Society), he was always ready to aid and assist in any investigations that might be required. And although the Council cannot but lament, in common with the Society, the partial separation of so active a member, yet they are well aware that the powerful resources of his mind will be more efficiently called into action, and will reflect more honour on his name and nation in the new sphere in which he is about to engage.

Previous to his departure for the Cape, Mr. Henderson made arrangements for observing Mars at his opposition in November next: and requested Mr. Baily to select the stars proper to be observed with that planet. This has been done; and the Council have directed that the same shall be printed and circulated amongst different observers, in order that the greatest advantage may be taken of the favourable opportunities now afforded for determining the parallax of that planet.

* At the anniversary in 1830, the Society presented Mr. Henderson with a copy of its Memoirs, as a small token of their sense of his services.

Arrangements had been previously made with Mr. Fallows for observing the parallax of the moon; and the stars proper for that purpose are now distinguished by an asterisk in the annual list of the moon-culminating stars: and as, in all probability, a proper selection of stars will be made and published in future, for the subsequent oppositions of Mars, and the inferior conjunctions of Venus, there is now every reason to hope that, with the cooperation of active astronomers, the parallaxes of these respective bodies will soon be determined with the greatest accuracy.

Besides the Observatory at the Cape of Good Hope, and the Observatories of Paramatta and Madras, we have also, in the southern hemisphere, another at the Island of St. Helena, founded by the East India Company, and placed under the superintendence of Mr. Johnson. Captain Lloyd also, who has been lately appointed Surveyor-General to the island of Mauritius, has taken out some excellent instruments to that island; and a $3\frac{1}{2}$-feet transit instrument, by Troughton and Simms, is about to be forwarded to the same place, at the expense of Government, to be placed under the superintendence of M. Dabadie, whose observations on the comet of 1830 were communicated to this Society. And Capt. King, R.N. is also shortly about to sail for New South Wales, provided with some astronomical instruments for his own private use. Thus, in the southern hemisphere, where a few years ago we could scarcely reckon a single observer, we have now several active and zealous persons, employed in watching the motions of the heavenly bodies; and in making such observations as may tend to advance the connected sciences of astronomy, navigation, and geography. And here the Council cannot avoid suggesting to those observers the great advantage that may be taken of their fortunate positions, by their forming arrangements with Mr. Henderson for making such observations, either of moon-culminating stars or such other celestial phænomena to be agreed on, as may tend to fix, on a firm and durable basis, the true longitudes of their respective stations: which will thus become so many zero points on the globe to which the navigator and the geographer may in all cases confidently refer.

The Meeting then proceeded to the Election of the Council for the ensuing Year, when the following Fellows were elected, viz.

President: John Brinkley, D.D. *Lord Bishop of Cloyne.—Vice-Presidents:* Francis Baily, Esq. F.R.S. L.S. and G.S. and M.R.I.A.; Davies Gilbert, Esq. M.P. V.P.R.S. F.L.S. & G.S.; John William Lubbock, Esq. M.A. V.P. & Treas. R.S.; Rev. William Pearson, LL.D. & F.R.S.—*Treasurer:* John Lee, Esq. LL.D.—*Secretaries:* Augustus De Morgan, Esq.; John Wrottesley, Esq. M.A.—*Foreign Secretary:* Captain W. H. Smyth, R.N. F.R.S. & A.S.—*Council:* George Biddell Airy, Esq. M.A. *Plumian Prof. Ast. University of Cambridge;* Charles Babbage, Esq. M.A. F.R.S. *Lucasian Prof. Math. University of Cambridge;* Captain F. Beaufort, R.N. F.R.S.; Lieutenant Thomas Drummond, R.E.; Captain W. F. W. Owen, R.N.; Lieutenant Henry Raper, R.N.; Edward Riddle, Esq.; Rev.

Richard Sheepshanks, M.A.; Lieutenant William S. Stratford, R.N.; Edward Troughton, Esq. F.R.S. L. & E.

XLVI. *Intelligence and Miscellaneous Articles.*

PREPARATION OF CAUSTIC POTASH. BY M. LIEBIG.

If one part of carbonate of potash be dissolved in four parts of water, and the solution be boiled with slaked lime, the potash does not lose the smallest quantity of carbonic acid; it does not become caustic, even though lime be added to any extent, or however long the boiling may be continued. If, however, 6 parts of water be gradually added to the above mixture, it will be found, and without further boiling, that the potash loses its carbonic acid gradually; and that after the addition of the last portion of water, the potash is perfectly caustic. If the water be added at once, the potash becomes very quickly caustic.

This peculiarity is explained by the fact, that concentrated caustic potash takes carbonic acid from lime. This fact is readily proved by boiling powdered chalk with concentrated potash, entirely free from carbonic acid; the solution added to muriatic acid occasions brisk effervescence. M. Liebig states that the carbonate of potash which is to be made caustic should be dissolved in at least 10 parts of water.

Ann. de Chim. et de Phys. xlix. *p.* 142.

ANALYSIS OF GUMS.

M. Guérin has analysed several varieties of gum with the annexed results. Arabin, which constitutes the greater portion of gum arabic, is composed of

Carbon	43·81
Oxygen........	49·85
Hydrogen	6·20
Azote..........	14
	100·00

The azote is considered as non-essential.

Gum arabic was found to consist of

Arabin	79·40
Water	17·60
Ashes..........	3·00
	100·00

Messrs. Gay-Lussac and Thenard found its composition to be:

Arabin	84·16
Water	13·43
Ashes..........	2·41
	100·00

The difference of water found depended upon the different methods

of drying: the gum in this analysis was dried at 212° in the air; while M.Guérin dried it at 257° *in vacuo*, which accounts for the larger quantity of water obtained by him. The quantity of ashes found by M. Guérin is the same as that procured by Vauquelin; they consist of carbonate of potash, chloride of potassium, oxide of iron, alumina, silica, and magnesia.

Gum Senegal.—100 parts of this gum treated with 500 of nitric acid gave 16·70 parts of mucic and oxalic acids. It is composed of

Arabin	81·10
Water	16·10
Ashes	2·80
	100·00

Its composition is therefore essentially the same as that of gum arabic.

Mucilage of Linseed.—The soluble part of linseed is composed of

Arabin and azotized matter	67·50
Water	14·00
Ashes	18·50
	100·00

Bassora Gum.—This gum swells much in water; treated with boiling alcohol it yields chlorophylle, a substance resembling wax, acetate of potash, chloride of calcium and supermalate of lime. It is composed of

Arabin	11·20
Bassorin	61·31
Water	21·89
Ashes	5·60
	100·00

Bassorin is solid, colourless, semi-transparent, insipid, inodorous, uncrystallizable, and difficult to powder. It is insoluble both in hot and cold water, but it absorbs it and swells considerably; it is also insoluble in alcohol, and does not undergo the vinous fermentation. 100 parts treated with 1000 of nitric acid gave 22·61 of mucic and oxalic acids. When treated with sulphuric acid it gives a crystallizable matter, which has a sugary taste, but does not form spirit by fermentation.

Bassorin is prepared by washing Bassora gum with a large quantity of cold water repeatedly, until it ceases to dissolve anything; the residue is then to be allowed to drain, to be dried in cloth, and the water is to be finally separated by exposure to a salt-water bath in a silver capsule.—Bassorin is composed of

Carbon	37·28
Oxygen	55·87
Hydrogen	6·85
	100·00

The soluble part of Bassora gum is similar to arabin; 100 parts of water at 68° dissolve 17·28 parts, and at 212°, 22·98 parts; 100

parts heated with 400 of nitric acid, gave 15·42 mucic acid and oxalic acid.

The soluble part, or arabin, of this gum gave by analysis:

Carbon	43·46
Oxygen	50·28
Hydrogen	6·26
	100·00

It is therefore evident that it is identical with arabin.

The insoluble portion of Bassora gum consists of bassorin mixed with phosphate of lime, silica, oxide of iron, and magnesia.

Gum Tragacanth.—Its sp. gr. is 1·384; when heated to between 125° and 145° Fahr., it is more easily powdered than at common temperatures. It swells prodigiously when put into water, and when boiled in water and treated with iodine, starch is shown to be present.

It is composed of

Arabin	53·30
Bassorin and starch	33·10
Water	11·10
Ashes	2·50
	100·00

Ann. de Chim. et de Phys. xlix. *p.* 248.

TRANSIT OF MERCURY OBSERVED AT GENEVA.

This interesting phænomenon was observed at Geneva by M. Gautier, on the 5th May, with a telescope of Dollond's, of 3½ inches aperture and 3½ feet of focal length. The power used was 72. The clock, one of Skelton's, was regulated to mean time at Geneva, and was about *seven* seconds fast.

	h	m	s
Mercury entered the Sun's disc at	9	24	57
Mercury had wholly entered on the disc at	9	27	40
First contact at the egress of the planet	4	10	17
Final contact	4	13	28

"The planet did not appear very distinct at the moment of its entrance, but it became so afterwards, particularly with a power of 135, and it presented a black and round disc, which was visible on a sheet of white paper placed before the eye-glass of the telescope without a darkening glass. M. Wastmann, who observed with a telescope of Fraunhofer's, of 30 lines aperture and a power of 120, threw the sun's disc upon paper, and observed that the disc of the planet was not black, but of a very distinct *reddish violet* colour, and without any penumbra. M. Eynard Chatelain measured the diameter of Mercury near the middle of the transit with a Troughton's micrometer adapted to a Ramsden's telescope of 27 lines aperture, and found it about 11½ seconds."—*Bibl. Univers. April* 1832, *p.* 431, 432.

ON THE EVOLUTION OF HEAT BY FRICTION AND PERCUSSION.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

To satisfy the inquiries of your Correspondent E., respecting the evolution of heat from iron by percussion, I immersed a small rod of iron in water contained in an iron basin, and struck it a few smart blows with a hammer; the end of the rod became hot as readily, though not so intensely, in water as in air.

As I have thus easily answered your Correspondent's inquiry, will you give me leave to inquire in your Philosophical work, first, Whether the heat thus evolved by percussion is not always accompanied by a corresponding loss of cohesion in the metal acted upon.

Secondly, Whether annealing, which restores its cohesion, is not simply restoring the heat evolved, or expressed from it by percussion, which increasing the specific gravity of the metal, decreases its capacity for heat.

Thirdly, Whether the great quantity of heat evolved by the friction of two metallic surfaces, under water, as observed by Count Rumford, and under oil, as is frequent in heavy machinery, is not strictly the effect of the destruction of cohesion, produced by abrasion; and whether heat is ever evolved under such circumstances, unless cohesion is destroyed.

August 13, 1832. X.

LUNAR OCCULTATIONS FOR SEPTEMBER.

Occultations of Planets and fixed Stars by the Moon, in September 1832. Computed for Greenwich, by THOMAS HENDERSON, *Esq.; and circulated by the Astronomical Society.*

1832.	Stars' Names.	Magnitude.	Ast. Soc. Cat. No.	Immersions.				Emersions.			
				Sidereal time.	Mean solar time.	Angle from North Pole.	Angle from Vertex.	Sidereal time. *	Mean solar time.	Angle from North Pole.	Angle from Vertex.
				h m	h m	°	°	h m	h m	°	°
Sept. 3	14 Sagittarii	6	2097	19 14	8 22	40	51	20 7	9 15	318	337
4	o Sagittarii	4·5	2205	18 31	7 35	78	74	19 54	8 58	284	294
8	50 Aquarii	6	2672	19 7	7 55	99	71	20 24	9 12	299	281
9	χ^1 Aquarii	5·6	2773	22 2	10 46	169	158	22 49	11 33	241	237
	ψ^2 Aquarii	5	2778	22 56	11 40	91	89	0 7	12 51	323	333
13	ξ^2 Ceti......	5	255	20 59	9 28	137	98	21 52	10 21	267	228
15	63 Tauri...	6	490	1 47	14 7	162	130	2 31	14 51	238	214
17	68 E^1 Orion.	6	768	22 7*	10 20	75	42	22 53	11 5	301	264

* Immersion in horizon.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by Mr.* Giddy *at Penzance, and Mr.* Veall *at Boston.*

Days of Month, 1832.	Barometer.					Thermometer.					Wind.			Rain.		
	London.		Penzance.		Boston	London.		Penzance.		Bost.	Lond.	Penz.	Bost.	Lond.	Penz.	Bost.
	Max.	Min.	Max.	Min.	$8\frac{1}{2}$ A.M.	Max.	Min.	Max.	Min.	$8\frac{1}{2}$ A.M.						
July 1	30·305	30·222	30·292	30·272	29·65	79	52	71	56	70	N.	S.	W.	...	...	...
2	30·207	30·165	30·192	30·172	29·57	76	47	71	56	66·5	NW.	S.	calm	...	...	...
3	30·142	30·064	30·072	29·992	29·53	75	49	70	55	63	E.	SW.	calm	...	...	...
☽ 4	30·015	29·957	29·942	29·942	29·40	79	52	71	55	62	E.	SW.	calm	...	...	...
5	30·006	29·977	29·948	29·892	29·39	79	52	70	55	65	W.	SW.	NE.	...	...	...
6	29·923	29·727	29·848	29·748	29·34	76	51	66	58	59	SW.	SW.	calm	0·03	0·960	...
7	29·925	29·726	29·748	29·748	29·15	72	58	66	54	62	W.	SW.	W.	...	...	...
8	29·900	29·830	29·778	29·748	29·30	72	59	68	57	62	SW.	SW.	W.	·01	·135	0·08
9	30·002	29·936	29·848	29·848	29·25	75	57	69	57	67·5	W.	SW.	W.	...	...	·02
10	29·966	29·820	29·828	29·748	29·30	72	56	65	59	67	S.	SW.	W.	·04	·410	·10
11	29·895	29·786	29·798	29·754	29·10	76	54	67	54	63	SW.	SE.	W.	...	...	·37
○ 12	29·887	29·724	29·804	29·654	29·27	80	57	66	55	66	S.	NE.	W.	...	...	·33
13	29·989	29·810	29·804	29·654	29·13	76	57	68	55	64	W.	NE.	W.	·72	·500	·30
14	30·249	29·999	30·348	29·848	29·40	75	47	64	57	64	NW.	NE.	W.	·07	·100	·15
15	30·407	30·354	30·407	30·398	29·77	74	54	65	51	64·5	W.	N.	NW.	...	...	·20
16	30·349	30·195	30·398	30·248	29·61	77	57	67	55	65	W.	NW.	NW.	...	...	...
17	30·182	30·077	30·254	30·148	29·50	84	55	67	57	66	W.	NW.	W.	...	...	...
18	30·124	30·092	30·151	30·148	29·45	70	45	65	57	60	NW.	NW.	NW.	...	...	...
19	30·194	30·179	30·144	30·138	29·60	67	41	63	52	56	NW.	NE.	NW.	...	...	...
☾ 20	30·218	30·155	30·154	30·098	29·67	69	43	63	50	57	W.	NE.	NW.	...	...	...
21	30·201	30·157	29·998	29·984	29·65	64	50	64	54	54·5	NE.	SE.	calm	...	...	...
22	30·243	30·210	30·054	30·048	29·65	64	42	67	52	55	NE.	NE.	calm	...	...	...
23	30·218	30·161	30·198	30·104	29·59	70	48	64	51	60	NE.	NE.	N.	...	...	...
24	30·254	30·123	30·204	30·198	29·60	70	53	64	54	60	NW.	N.	N.	...	...	...
25	30·221	30·171	30·254	30·248	29·56	75	48	65	55	62	W.	NE.	NE.	...	...	...
26	30·164	30·132	30·101	30·098	29·47	72	47	66	55	62	E.	N.	calm	...	...	...
● 27	30·161	30·134	30·104	30·098	29·51	69	46	68	54	60	NE.	NE.	calm	...	...	...
28	30·292	30·224	30·142	30·101	29·65	73	47	69	55	62·5	NE.	NE.	E.	...	...	...
29	30·386	30·348	30·192	30·186	29·76	73	44	69	55	65	NE.	NE.	NW.	..	...	...
30	30·390	30·347	30·215	30·206	29·83	72	48	69	54	62	E.	SE.	E.	...	...	...
31	30·323	30·188	30·195	30·092	29·76	66	46	68	55	61	NE.	SE.	calm	...	...	...
	30·407	29·724	30·407	29·654	29·49	80	41	71	50	62·3				0·87	2·105	1·55

Remarks.

London. — July 1. Sultry. 2. Fine. 3. Hazy: sultry. 4, 5. Fine. 6. Cloudy: rain. 7. Cloudy: fine. 8—10. Cloudy. 11. Slight showers: fine. 12. Fine: thunder-showers at night. 13. Fine. 14. Heavy rain. 15—21. Fine. 22. Fine: cold for the season at night. 23, 24. Overcast. 25—29. Fine. 31. Overcast and fine. The quantity of rain which fell in this month is little more than one third of the average for the same period of the season.

Penzance.—July 1—3. Clear. 4, 5. Fair. 6. Rain. 7. Showers: fair. 8. Misty: fair. 9. Fair. 10. Rain. 11. Fair: rain. 12, 13. Rain: fair. 14. Rain: clear. 15. Clear. 16. Fair: misty. 17, 18. Fair. 19. Fair: clear. 20. Clear. 21. Fair: clear. 22. Clear. 23, 24. Fair: clear. 25. Clear. 26. Clear: fair. 27. Clear. 28. Fair. 29, 30. Clear. 31. Fair.

Boston.— July 1. Fine. 2—4. Cloudy. 5. Fine. 6, 7. Cloudy. 8. Rain. 9. Fine: rain early A.M. 10. Cloudy. 11. Rain: with thunder and lightning A.M., and again at noon. 12. Fine: rain P.M. 13. Cloudy: rain P.M. 14. Cloudy: rain A.M. 15. Fine. 16. Cloudy. 17. Fine. 18, 19. Fine: brisk wind. 20. Fine. 21, 22. Cloudy. 23. Fine. 24—27. Cloudy. 28, 29. Fine. 30, 31. Cloudy.

THE

LONDON AND EDINBURGH

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[THIRD SERIES.]

OCTOBER 1832.

XLVII. *Investigation of certain remarkable and unexplained Phænomena of Vision, in which they are traced to Functional Actions of the Brain. By* Mr. THOMAS SMITH, *Surgeon, Fochabers**.

SOME years ago I published in this Journal† an account of a remarkable affection of sight which I had discovered accidentally while engaged in a course of experiments on the apparent luminousness of objects, and which was thought the more worthy of being recorded, that it could be easily produced in others as well as in myself, by placing the two eyes in certain relations to light.

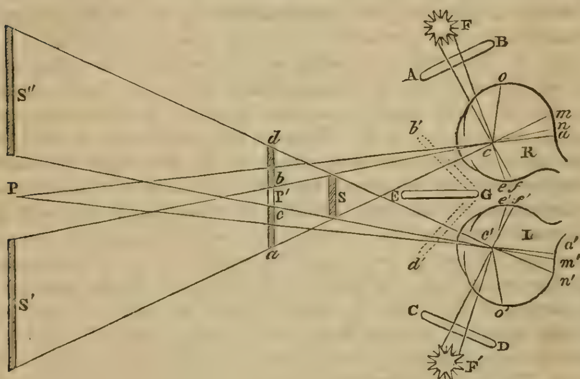
My former communication having been limited to a mere announcement of the new facts I had observed, I propose, in this paper, to state the results of the attempts which I have made, since that period, to discover the causes of the phænomena. These results might be mentioned in a few words; but as the subject is, in almost all its bearings and relations, entirely new, and as the conclusions to which I have been led, tend, if confirmed, to throw a very unexpected light on the nature and exciting causes of *cerebral function*, of the laws of which so little is known, I have thought it incumbent on me to give each particular step of the investigation in detail, to enable the reader who takes an interest in the subject, to judge how far the conclusions are borne out by the premises. Our total ignorance of the proximate or immediate causes of per-

* Communicated by the Author.

† See Edinburgh Journal of Science, First Series, vol. v. p. 52.

ception in general, the uncertain state of our knowledge of any function of sight beyond the mere formation of the image on the retina, will, I trust, be deemed an apology for the minuteness of research and detail into which I have entered. My wish has been to advance nothing which is not founded on accurate observation, or experiment and legitimate induction.

In entering upon the investigation, it is of much importance to form a correct conception of the optical relations of the eyes at the time of making the experiments, and of the several objects concerned in them. For this purpose, and to make the phænomena themselves better understood, a diagram will



be useful. Let R and L, represent the right and left eyes; P a distant point to which they are both directed; S a narrow slip of white paper placed vertically between that point and the eyes, and about ten or twelve inches from the latter: F the flame of a lamp or candle near the right eye, and so placed that rays from it may enter that eye freely, while they are excluded from the left by an intervening obstacle E G; and F′ an equal flame placed near the left eye.

By the principles of optics, we know that straight lines drawn from the objects through the optical centres of the eyes, viz. c and c', will touch those parts of the retina in each eye on which the several images fall. Hence $e\,f$ represent the place of the image of F in the right eye; $e'\,f'$ that of the image of F′ in the left; $m\,n$ and $m'\,n'$ the places of the images of S; and o and o' the parts where the light from F and F′ would strike, if not prevented by the screen E G.

It is a law of vision, that objects are perceived in the direc-

tions of right lines drawn from their images on the retina through a certain point which has been termed the *centre of visible direction.* Hence supposing c and c' to be these points in the two eyes, the slip of paper S, by the operation of this law, will be perceived by the right eye somewhere between the lines $m\,c$, $n\,c$ produced, and by the left eye somewhere between $m'\,c'$, $n'\,c'$ produced. If S appears at the distance of the point in which the axes of the two eyes meet, then two images S′ and S″ will be seen when the eyes are directed to P; the two images will overlap each other when the axes meet in P′, and they will coalesce into one object when the eyes are directed to S.

These things being premised, we shall now proceed to state, as briefly as possible, the experiments by which the phænomena that require particular attention may be exhibited.

Exp. 1. The two eyes being equally sensible to light, and the two lights F and F′ being placed as in the figure, make the slip of white paper S appear double, by directing both eyes to a distant point P. Between one of the eyes L and the light next it interpose the screen C D, the right eye alone being exposed to the direct light F. As soon as this unequal exposure of the two eyes is made, the two images of S will be observed to change their colour; S′, which is seen by the exposed eye, becoming *green*, and S″, which is seen by the shaded eye, becoming *red*. Withdraw the screen C D and interpose the screen A B; when L, which is now the exposed eye, will see the image S″ change from *red* to *green;* while S′, which is now the image seen by the shaded eye, will change from *green* to *red*. The same phænomena occur, and are even more vivid, if one of the eyes is exposed to the rays of the sun either direct or reflected.

Exp. 2. In the first experiment the images of S fall on the retina out of the axes of the eyes a and a'. If this circumstance should be suspected to have any part in producing the green and red appearances, direct the two eyes to S, thus bringing the two images $m\,n$, $m'\,n'$ into the axes. When S is seen by both eyes at once, in this case it appears of its proper white colour; but if the screen E G is turned alternately to the positions G b', G d', so as to make S visible to only one of the eyes at once, then it will be found to appear *green* to the exposed eye, and *red* to the shaded one.

This observation is an important one in another point of view, as it proves that the red and green colours of the two images are complementary to one another, a fact which is demonstrated otherwise by directing the two eyes to P″; for the part $b\,c$, where the two images overlap each other, appears white, though the rest of them $a\,c$, $b\,d$, appear red and green.

Exp. 3. When both eyes, directed to P, are shaded by means of the screens A B and C D, the two images S′ and S″ appear *white*. The same is true when both eyes are exposed by withdrawing the screens; but a difference in the degree of their whiteness occurs in these two circumstances, which is too important to pass unheeded. When both eyes are shaded, having attended well to the degree of whiteness which the two images of S assume, suddenly withdraw both of the screens; and though the two images will still seem white, yet they will appear sensibly darker than before, as if a thin cloud had been suddenly interposed between them and the source of their illumination. By interposing the screens again, their lustre is suddenly and permanently restored.

Exp. 4. To ascertain through what portion of the retina this extraordinary affection of sight occurs, I made the image of the slip of white paper S traverse the retina of the exposed eye, from the borders of the bright image *e f* to *o* and the corresponding parts of the shaded eye; but I could not observe that the green appearance to the exposed eye and red appearance to the shaded eye, was less at any one point than another. If the bright image fell on the centre of one retina, every part of the retina around that part, both in it and the other eye, appeared, as far as the image of S remained perceptible, under the operation of this affection.

These four experiments illustrate the principal facts that require to be kept in view. It is obvious that the *green* appearance of the white paper S to the exposed eye, and the complementary *red* colour which it assumes to the other eye, are phænomena which admit of explanation in two ways; for they may be accounted for either by an existing *excess of sensibility* to the *apparent* colour, or by a *defect of sensibility* to its *complementary* colour. The first step, therefore, which I took in this stage of the inquiry into the causes of the phænomena, was, *to investigate the precise nature of the effects produced by the action of the bright light* F *on one eye.* For this purpose I made the following experiments.

Exp. 5. Having painted a narrow piece of white paper as nearly as I could of the *green* colour which the white slip S assumed to the exposed eye in the first experiment, I substituted it in the place of S; and when my two eyes were directed to P, I observed that the left-hand image S′, which was seen by the exposed eye, appeared of a *deep green colour*, and the right-hand image S″, seen by the shaded eye, of a *bright whitish colour*. When the two images were made to coalesce partially by directing the eyes to P′, the part *b c*, in which both colours united, appeared of that degree of green colour which I had given to S.

From the latter part of this, and also from the second experiment, an important piece of knowledge is derived; namely, that *whatever be the state of the sensibility in the one eye, that state is exactly reversed at the same time in the other eye.* Hence, if the *deep green* appearance of S′ in this last experiment is owing to an excess of the perception of the green light, the *bright whitish* appearance of S″ must be derived from a defective perception of the green light. We know that the green colour of the slip of paper used in this experiment was not owing to an absolute defect of the red and other primary coloured rays, but to an excess or predominance of the green light reflected from it. The whitish appearance of S″, therefore, might be the result either of insensibility to this excess of green, or of an increase of sensibility to the light which is complementary to green. The whitish appearance, therefore, observed by the shaded eye in the fifth experiment is altogether equivocal, unless strict attention is paid to its *degree*, as will be manifest from the following considerations. *White* is the result of a mixture of any two antagonist or complementary colours in certain proportions. Let us, for the sake of illustration, suppose these proportions to be equal; then one part of *red light* and one part of *complementary green*, or two parts of each, or, in short, any equal parts of each, will form *white*, the brilliancy of which will be as the number of rays composing it. Now suppose a surface which appears *green* to contain one part of *red* and five of *green*, it is obvious that if this surface is converted to white by an addition of *red light*, it will be five times brighter than if it is made white by a removal of *green light*. The white colour of the green slip to the shaded eye, in the fifth experiment, appeared to me much brighter than could be accounted for by insensibility to the superabundant *green light*. But to remove all uncertainty on the subject, I had recourse to the following experiment.

Exp. 6. I laid on a deep black ground a slip, about one twentieth part of an inch in breadth, of the same green paper which I had used in the last experiment, in a straight line with a slip of white paper of the same breadth. I viewed them both through a prism held parallel to their length, and observing that the *red* margin appeared much broader and brighter in the white than in the green slip, I shaded the white one with China ink, by degrees, until the red border in it appeared exactly like that of the green slip, expecting to obtain in this manner a very correct specimen of the degree of whiteness which the green slip used in the last experiment would assume, if the eye was insensible to the predominant green rays: that it was so, there cannot, on reflection, be any doubt. I therefore used this shaded slip as *a test* on repeating

the fifth experiment; and found that, compared with it, the image S″ seen by the shaded eye was incomparably brighter: whence it is quite certain that the sensibility is *increased to red*, and not impaired to green in the unexposed eye.

As the sensibility being *increased* to red light is the cause of the appearance to the shaded eye, it follows, from what has been remarked above, that the appearance to the exposed eye arises from *equally diminished* sensibility to the same colour. As this induction, however, may not appear so complete as it ought to be, on account of the phænomena mentioned under the third experiment, the state of the sensibility to red light in the exposed eye may be determined by direct experiment, thus:—

Exp. 7. Having found, by examination with the prism, that *red morocco leather* reflected few or none of the green rays when exposed to moderate white light, I put a slip of it in the position S, expecting that the image seen by the exposed eye would appear very dark or nearly black if the sensibility was remarkably diminished to red, but that it would appear *bright white* if the sensibility was increased to green: the result was, that it appeared a very dark red, and even (when the light F was very brilliant) almost black.

Although these experiments appear to establish the truth of the conclusion, that the *green* and *red* appearances of the white surface S, in the first and second experiments, were owing to changes in the sensibility to, or perception of, *red light alone;* yet it is proper that we should pause here, to consider the fact disclosed by the third experiment, viz. that the two images of S appeared *whiter* when the two eyes were shaded from, than when they were both exposed to, the same quantity of bright light F and F′. The result in the third experiment may be thus expressed—*diminished sensibility to white light by the equal action of bright light on both eyes.* Now the results obtained by the first and second experiments have been proved to arise from the sensibility to red light being diminished in the exposed eye, and increased equally, as appeared, in the shaded eye: the effect, therefore, to be expected from exposing both eyes, is decreased, and equally increased sensibility to red light in each eye, or, in other words, an unchanged state of the perception of the white of both images of S,—a result manifestly at variance with that of the third experiment. But this was not the only perplexing circumstance encountered in this stage of the inquiry; for in repeating the experiments with white, red, and green slips of paper by turns, it could not long escape observation, that a *partial coalescence* of the two images, as at *b c*, gave a *brighter colour* than a *total coalescence* at S. Happily, however, an

affection of vision, which I had discovered several years before, and even laid before the public (though in a manner that appears to me very unsatisfactory now), afforded a key to these difficulties.

It was commonly taught, and, if I am not mistaken, still continues to be so, that in any pre-existing state of the sensibility of the eye, the apparent luminousness of an object is in the direct ratio of the quantity of light from any point of it entering the eye and acting on any point of the retina. Physiologists had been led, by certain appearances, to adopt as a principle, that the sensibility of the retina is fatigued, impaired, or exhausted by the continued action of strong light; but no one had ever suspected that the sensibility could be augmented in consequence of the action of bright light, or that it could suffer a diminution without the action of light to account for it: yet I ascertained, by a series of unequivocal experiments, that, in certain circumstances, *the action of a stronger and weaker light on adjoining parts of the retina was attended with an increased perception of the colour of the brighter object, and a diminished perception of the same colour in the darker one**. Attention to

* Some important errors having crept into the account which I formerly published of this affection of sight, I find it necessary, instead of referring to it, to explain here the facts by which the existence of the principle announced in the text appears to be demonstrated.

Take six pieces of thin white writing-paper, about four inches square each, and having painted them of the principal *primary* colours, viz. one *red*, another *orange*, a third *yellow*, a fourth *blue*, a fifth *green*, and the sixth *violet*, form them into *tubes*, to be used as will be directed immediately. Form also two other tubes of the same size,—one of them of thin white writing-paper, the other opake and blackened within.

Now if one of the coloured tubes is applied to one of the eyes, the other eye being naked, and if the tube be strongly illuminated, a white surface, visible to both eyes, will present the following remarkable appearances. The round spot of the white surface that is seen through the tube will appear, not white, but uniformly of the colour that is complementary to that of the tube; while the rest of it will appear of its proper white colour to the naked eye. Thus if the tube employed is *red*, the circular spot seen through it will appear *green*; through the yellow tube it will appear *purple*; through the *blue* tube *orange*, &c.; and the power of the colour of the tube to affect the perception of a white surface seen through it may be still further illustrated by using tubes of different shades of the same *primary* colour. If two such tubes, differing only in degree, be applied to the two eyes at once, and directed to different parts of the white surface, the two circular spots thus seen will be as different in the degree of the complementary colour as the tubes are in the primary colour. If we attend to the optical state of the eye during these experiments, it will be found that every part of the retina, except a small circular spot towards the middle of it, is acted upon by the coloured light from the tube, and that this circular spot is acted upon by the white light from the surface seen through the tube. How is it, then, that we do not see the circular spot white, but of a colour which constantly varies so as to be complementary

the second experiment soon convinced me that the action of the stronger lights F and F′, and of the weaker light from S, and all surrounding objects, placed the retina in precisely the same condition in which the affection now mentioned had been developed in the experiments referred to. This principle, therefore, accounted for the two images of S appearing darker when the eyes were exposed to, than when they

to that of the tube? M. Meusnier observed that, when the sun shone through a hole a quarter of an inch in diameter in a red curtain, the image of the luminous spot was green. M. Meusnier's retina in this case was evidently in a state closely analogous to that of the eye looking through a tube in any of the above instances. It has been proposed to explain the false perception in M. Meusnier's case on the principle, "that when the whole, or a great part, of the retina has the sensation of any primitive colour, a portion of the retina protected from the impression of the colour is actually thrown into that state which gives the accidental or harmonic colour."—(*Vide* Lardner's Cyclopædia, "*Optics*," p. 310.) If this principle exists, the appearance of the white surface seen through any of the coloured tubes would be owing to an *increase of sensibility* to the complementary colour in that part of the retina where the white light acts; but till direct proof of that principle is obtained, it cannot be denied that the phænomena would be equally well accounted for, by supposing the *sensibility diminished* to those rays of the white surface, which are of the same colour as the tube; for a white surface will appear as distinctly *green* by the abstraction of *red* as by the addition of green. To determine, therefore, to which of these causes the phænomena were due, I adopted the following plan.

As *whiteness* is the result of the united action of all the primary coloured rays, so a *white* tube may be regarded as an assemblage of all the primary coloured tubes into one. Hence, in viewing a white surface through a *white* tube, we ought to have a combination of all the effects that are obtained on viewing the same white surface through the primary coloured tubes separately. Consequently, if the green appearance of a white surface seen through a *red* tube is owing to an increase of the sensibility to green, and so on of the others, then it is obvious that a union of all these *increased sensibilities* will constitute an *increased sensibility to white*; and, therefore, that a white surface, viewed through a white tube, ought to appear brighter than it does to the naked eye. But if the appearance of the white surface through each coloured tube is owing to *diminished sensibility* to those rays from the white surface that give the same colour as the tube, then a union of all the diminished sensibilities will form a *diminished sensibility to white*, consequently make the white surface seen through the white tube appear darker than it does to the naked eye. Experiment declares unequivocally in favour of the latter state of the sensibility; for the circular area of the white surface, which was seen through a white tube highly illuminated, appeared exceedingly dark, and if the illumination of the tube was very brilliant, almost black, compared with the other parts of it, which were seen of their ordinary white colour by the naked eye. In this manner, therefore, we are enabled to pronounce with certainty, that the false vision of the white surface seen through a coloured tube, is owing to the sensibility being diminished to the action of that kind of rays in the white image, which predominates in the tube employed.

By a proper management of the illumination of the white tube and white surface seen through it, we soon find that *this diminished sensibility* is only to *a weaker light in the vicinity of a stronger*; and further, *that there*

were shaded from, the bright light, and thus reconciled the result of the second experiment to that of the others. But when the exciting cause of this affection (See Note*, p. 255, &c.) is understood, when it is known that it occurs only when one of the objects is seen *indistinctly* from any cause (such as the image falling on the retina before or behind the focal point of the rays composing it)†, all difficulty in regard to the brighter

always occurs at the same time an increase of the sensibility to the stronger light next to a weaker; for when the white tube is strongly, and the white surface weakly, lighted, the appearances of the latter are always such as I have stated above, to the two eyes. If the lights are now placed so as to make the illumination of the tube and surface more equal, the difference to the two eyes becomes less and less, till when both tube and surface are equally illuminated, the area seen through the tube appears as white as the rest of it does to the naked eye—a satisfactory proof, as appears to me, that *the sensibility is only diminished to the weaker light in the vicinity of a stronger.* But if the lights are so placed as to illuminate the white surface more than the tube, the former begins to appear brighter through the tube than to the naked eye; and when the difference of illumination is very great, as when we view a white surface through a *black* tube, the surface, which appears of its proper brightness to the naked eye, will appear over the circular part seen through the tube, as if the sun shone upon it and not upon the rest,—a decided proof that *the sensibility is increased to the stronger light in the vicinity of a weaker.* That the increase and decrease of sensibility here demonstrated occur at the same time and in the same degree, may be shown by carefully performing the following experiment. Take two pieces of paper, about four inches long and two inches broad each, the one bright white, the other painted dark, but not entirely black. Join the two pieces together so as to make one square piece, and at right angles to the line of their junction draw three straight lines with black ink through the white division, one of the lines being in the middle of it, and the other two at the distance of about three quarters of an inch from it on each side. Direct the two eyes to any distant point, and without altering their direction, introduce the square piece of paper between the eyes and the point they are directed to, holding the line of junction of the white and dark parts horizontally, and so that the two outer black lines shall appear united through their whole length with the middle black line. If strict attention is now paid to the appearance of the white and dark divisions, it will be found, if the experiment is properly performed, that the white portion will appear to terminate in the dark one by a very bright belt; while the dark portion will appear at the same time to terminate in the white one by a darker or black belt of the same breadth, and both of these belts will be seen to have evanescent edges at the sides where they disappear in their respective surfaces. These appearances are manifestly results of the same affection of sight that occurs in looking through coloured tubes, and show clearly that the increased sensibility to the greater light, and the diminished sensibility to the lesser, in the vicinity of each other, take place at the same time and in the same degree, the one being dependent upon the other.

† If a *white* wafer or round piece of *white* paper is steadily viewed on the middle of an extensive *red* ground, about a foot from the eyes, the retina will evidently be in the same physical condition in regard to the light falling on it, as when a *white surface* is viewed through a *red tube* duly illuminated, except that in the latter case the line of junction between

appearance of the two images of S when they coalesce partially, as at *b c*, than when they coalesce totally at S, will vanish; for the image on the retina is *distinct* in the latter case, and *indistinct* in the former.

Allowance being made, therefore, for the effects on the sensibility produced by the action of a greater light in the vicinity of a lesser, the results in all the preceding experiments will be found to be in strict accordance with the following conclusion: viz. *that the green appearance of a white object through all parts of the retina around the bright image in the exposed eye is owing to diminished sensibility to red light; and the red appearance of a white object through all the corresponding parts of the shaded eye is owing to an equal increase of the sensibility to the same kind of light.*

[To be continued.] p. 363

he red and white images is rendered indistinct by the scattered red rays; whereas in the former, both the surfaces and their line of junction are perfectly distinct: but when the eyes are fixed steadily on the centre of the wafer, it never fails to be seen of its proper white colour as long as it is seen without any dazzling or fatigue of the eyes. I fell into an important error in my first experiments on this subject, and thus was led, in my first publication adverted to in the text, to a misstatement of this fact, having too hastily averred that these changes in the sensibility took place *in every case* in which a greater and a lesser light were viewed together. I had no suspicion that indistinctness of vision could have anything to do in producing the changes; and therefore when, on fixing my eyes on the centre of a white circle on an extensive *red* ground till they became dazzled and fatigued, I saw the white circle appear green, I concluded, erroneously indeed, but perhaps not unnaturally, that the change in the sensibility from which the green appearance arose, had actually commenced the moment I fixed my eyes on the white circle. So convinced, indeed, was I, for a long time, of the correctness of this conclusion, that I was not a little surprised and disappointed, on submitting my observations to several friends, to find that though they unanimously agreed with me respecting the appearances of a white surface viewed through coloured tubes, yet not one of them could be got, without prompting, to declare that a white circle on a wide coloured ground presented the same appearances. At length, on finding that I myself never experienced the changes in the sensibility but when the object became indistinct, I was led to attend to the importance of that condition, which enabled me to reconcile my own experience with that of intelligent persons who formerly differed from me. The power of indistinctness to excite this affection is well illustrated by the effects of *twilight*, which is well known to throw a shade of indistinctness over objects. I lately witnessed a very striking instance of this. Some *white* and *pink-coloured* paper were lying together on my table one evening, when the light of day was just enough to show the colour of the red paper. One of the company present said, pointing to the *white* paper, What a beautiful green colour that is! and every person present agreed that it was so, though I assured them it was actually white: and not a little surprise was excited when it was distinctly perceived to be so, after I had removed the pink paper. A white image, reflected by *coloured* glass, appearing of its *complementary* colour, is also a fine instance of this effect of indistinctness.

XLVIII. *Note on the Mean Temperature of Sevastopol, as deduced from the Observations of* M. Coumani. *By Professor* M. A. Kupffer, *of the Imperial Academy of Sciences of St. Petersburg**.

SEVASTOPOL, where the following observations were made, stands on the western shore of the Peninsula of the Crimea, and is situated in north latitude 44° 35′½, and in longitude 33° 32′ east of Greenwich.

The months are reckoned according to the Julian Calendar, or Old Style, and the observations were made twice a day, at 10^h A.M. and 10^h P.M., and at the hours of maximum and minimum.

Table I.—*Mean State of the Octogesimal or Reaumur's Thermometer at Sevastopol, in the Years* 1827—1830.

	1827.		1828.		1829.		1830.	
	Mean of 10^h A.M. and 10^h P.M.	Mean of Maxim. and Minim.	Mean of 10^h A.M. and 10^h P.M.	Mean of Maxim. and Minim.	Mean of 10^h A.M. and 10^h P.M.	Mean of Maxim. and Minim.	Mean of 10^h A.M. and 10^h P.M.	Mean of Maxim. and Minim.
January	+ 2°·9	+ 2°·7	− 3°·5	− 3°·3	− 0°·9	− 0°·7	− 0°·8	− 0°·7
February	3·8	3·4	+ 3·0	+ 3·0	+ 2·7	+ 3·0	+ 0·3	+ 0·2
March	5·8	5·8	7·0	7·3	7·3	6·8	3·9	3·9
April	9·7	9·7	9·8	9·9	11·3	10·9	8·8	8·7
May	15·3	14·8	14·2	14·1	13·3	12·9	15·5	15·6
June	19·4	19·1	19·2	18·8	16·0	15·9	17·7	17·4
July	19·3	18·3	19·1	18·5	18·8	18·8	18·7	18·3
August	17·4	16·9	15·8	17·2	17·2	17·2	18·4	18·0
Septemb.	12·7	12·8	11·9	11·9	14·6	14·7	13·0	13·3
October	10·2	10·3	5·9	5·9	6·9	7·0	6·2	6·3
Novemb.	6·3	5·1	3·8	3·0	0·7	0·8	5·8	6·0
Decemb.	+ 1·3	+ 1·2	+ 1·5	+ 1·8	+ 1·0	+ 0·9	+ 5·4	+ 5·3
Means	+10·2	+10·0	+ 8·9	+ 9·0	+ 9·1	+ 9·0	+ 9·4	+ 9·4

	Reaumur.	Fahr.
Mean Temp. of 1827—1830, at 10^h A.M. and 10^h P.M.	+9°·4	53°·15
Mean Temp. at the hours of Max. and Min.	+9 ·35	53 ·04

Table II.—*Mean Temperature of a Spring at Sevastopol, in the Years* 1827, 1828, *and* 1829.

	1827.	1828.	1829.		1827.	1828.	1829.
January ...	+ 8°·2	+ 7°·6	+ 8°·5	July	+11°·2	+11°·6	+12°·8
February..	7·9	8·2	9·0	August.....	11·5	11·9	12·4
March ...	8·2	9·1	9·6	September	11·4	11·3	12·4
April	9·1	10·7	10·9	October....	11·0	10·2	10·6
May	10·2	11·8	12·4	November..	10·1	9·8	8·1
June......	10·4	+11·7	+13·2	December..	+ 9·0	+ 8·2	+ 7·8

Mean for 1827 .... +9° ·9
Mean for 1828 .... 10 ·2
Mean for 1829 .... 10 ·6
Mean for *three* years.......... 10·23 Reaumur.

* Communicated by the Author.

TABLE III. *Extreme Variations of the Octogesimal Thermometer at Sevastopol, in each Month of the Years* 1828—1830.

	1828.			1829.			1830.		
	Max.	Min.	Diff.	Max.	Min.	Diff.	Max.	Min.	Diff.
	°	°	°	°	°	°	°	°	°
January...				+ 8·0	−14·7	22·7	+ 9·3	−13·4	22·7
February	+16·3	− 6·0	22·3	11·9	7·6	19·5	7·7	7·6	15·3
March ...	21·4	− 2·6	24·0	21·3	− 2·4	23·7	11·4	− 3·5	14·9
April......	18·0	+ 2·0	16·0	18·5	+ 4·7	13·8	20·6	+ 2·0	18·6
May	23·3	5·0	18·3	26·0	3·8	22·2	23·7	8·4	15·3
June	27·0	11·3	16·3	25·3	8·0	17·3	26·8	11·6	15·2
July	25·8	11·3	14·5	27·2	11·2	16·0	27·7	10·0	17·7
August ...	29·9	7·3	22·6	25·6	10·4	15·2	26·8	11·0	15·8
September	21·0	+ 4·8	16·2	23·4	+ 9·4	14·0	21·6	+ 6·6	15·0
October...	14·1	− 0·7	14·8	18·6	− 1·9	20·5	15·4	− 1·1	16·5
November	11·2	9·6	20·8	12·7	8·9	21·6	13·3	0·0	13·3
December	+ 7·0	− 8·0	15·0	+11·8	− 9·7	21·5	+13·8	− 7·0	20·8

TABLE IV.—*Greatest Variation of the Octogesimal Thermometer at Sevastopol, on the different Days of each Month of the Years* 1828, 1829, 1830.

	Greatest Variation in a Day.				Greatest Variation in a Day		
	1828.	1829.	1830.		1828.	1829.	1830.
	°	°	°		°	°	°
Jan. ...		13·0	10·1	July ...	12·6	11·2	13·7
Feb. ...	13·1	13·2	7·4	August	13·9	12·1	12·2
March	16·1	11·7	9·8	Sept. ...	10·9	11·6	10·2
April ...	10·8	10·8	12·2	Oct. ...	7·4	9·4	9·8
May ...	11·0	16·1	11·7	Nov. ...	8.4	9·3	9·6
June ...	13·2	14·1	11·6	Dec. ...	8·0	10·0	+11·6

TABLE V.—*The Means of the Maxima and Minima of each Day, for every Month of the Years* 1828—1830.

	1828.			1829.			1830.			
	Mean of Max.	Mean of Min.	Diff.	Mean of Max.	Mean of Min.	Diff.	Mean of Max.	Mean of Min.	Diff.	Mean of the Diff. for the Three Years
	°	°	°	°	°	°	°	°	°	°
Jan. ...				+ 2·0	− 3·5	5·5	+ 1·8	− 3·2	5·0	5·3
Feb. ...	+ 6·2	0·0	6·2	5·0	+ 0·8	4·2	2·7	− 2·3	5·0	5·1
March	10·3	+ 4·0	6·3	9·4	4·2	5·2	6·6	+ 1·2	5·4	5·6
April ...	13·9	5·9	8·0	14·4	7·4	7·0	12·1	5·3	6·8	7·3
May ...	18·3	9·8	8·5	16·9	8·7	8·2	19·4	11·7	7·7	8·1
June ...	22·9	14·6	8·3	20·1	11·6	8·5	21·0	13·7	7·3	8·0
July ...	22·4	14·7	7·7	22·9	14·4	8·5	22·2	14·3	7·9	8·0
August	21·2	13·2	8·0	21·3	13·0	8·3	21·8	14·2	7·6	8·0
Sept. ...	15·0	8·7	6·3	18·6	10·7	7·9	16·5	10·2	6·3	6·8
Oct. ...	8·6	3·3	5·3	10·0	+ 4·1	5·9	9·1	3·6	5·5	5·6
Nov. ...	5·8	+ 0·3	5·5	3·1	− 1·4	4·5	8·2	3·9	4·3	4·8
Dec. ...	+ 3·2	− 1·1	4·3	+ 3·2	− 1·4	4·6	+ 8·0	+ 2·8	5·2	4·7

[*Observations on the preceding Results* (see Lond. and Edin. Phil. Mag. and Journ. No. 2. p. 135).

It appears from the first of the preceding Tables, that the mean temperature of Sevastopol for *four* successive years, from

1827—1830, at 10^h A.M. and 10^h P.M. is 9°·4 Reaumur, or 53°15 Fahr. When we correct this result by +0·122, the quantity by which the mean of 10^h and 10^h differs from that of the 24 hours, we obtain,

	Fahr.
Corrected mean temp. of Sevastopol	53°·272
Mean temp. calculated by formula T = 86°·3 sin. D − 3°½*, D the dist. from the Asiatic Pole being = 41° 22′	53 ·534
Difference between the formula and the observation	+ 0°·262

The coincidence is here very striking, the difference amounting only to a quarter of a degree of Fahrenheit. D. B.]

XLIX. *Further Experiments with a new Register-Pyrometer for Measuring the Expansion of Solids.* By J. Frederick Daniell, *Esq. F.R.S. Professor of Chemistry in King's College, London.*

[Concluded from page 204.]

I NOW arranged the two bars in two registers; and having strongly heated the furnace and filled the air-chamber itself with coke, I cleared out a space in which they could be placed, without coming in contact with the fuel on each side of them. Their two ends rested on pieces of fire-brick; the wrought-iron was placed lowest, and, the thickness of the register, in advance of the cast-iron; which was placed about two inches higher. The apertures were now all closed, and the draught increased to the utmost. At the expiration of a quarter of an hour the register with the cast-iron was removed with a pair of tongs; and the metal upon lifting it, immediately flowed out at the two end holes. The register, with the wrought-iron was then taken out. The bar of the latter was found perfect without any signs of oxidation or fusion.

The arc measured of the cast-iron was	9° 47′
The arc of the wrought-iron....................	7 56

I had some reason to think that the register, with the wrought-iron bar, had not been exposed so fully to the heat as that with the cast-iron: for, although placed slightly in advance of the latter towards the body of the furnace, it was not raised so high from the floor of the flue, which probably had a cooling influence; and as the flame was drawn upwards, it must have struck with greater force upon the higher register. I therefore replaced the wrought-iron bar in the register, and

* In this formula in Number 2. p. 136, place the comma that is after the second D, before it.

put it exactly in the position previously occupied by the cast-iron; it was then covered with charcoal, and the fire urged to the utmost. At the expiration of twenty minutes it was removed: the bar was found uninjured, with a white metallic lustre, except over the apertures, where it was blue, and perfectly free from oxide. The arc now, however, measured 11° 16′.

Now from these experiments there are four ways of approximately determining the temperature of melting cast-iron.

1st. By taking the expansion of cast-iron to its melting point, and calculating from the expansion for 150° to the boiling point of water, upon the supposition that the same rate is maintained, and adding the initial temperature of 60°, we obtain 3096°.

2ndly. By calculating from the expansion of the same bar for 600° to the boiling point of mercury, supposed equal, we obtain 2489°.

3rdly. By assuming the expansion of a bar of wrought-iron, at the point of melting cast-iron, and calculating from the expansion of the same bar for 150° to the boiling point of water, we obtain 2957°.

4thly. By calculating from the expansion of the same bar for 600° to the boiling point of mercury, supposed equal, we obtain 2533°.

It is remarkable that the mean of these four determinations is 2768°; for it will be remembered that the corrected temperature, which I deduced from the expansion of a platinum bar plunged into melting cast-iron, was 2786°.

It may be observed, that in both cast-iron and wrought-iron, the calculation from the rate of expansion to the boiling point of water gives a temperature higher than the true; and that, in both, the calculation from the point of boiling mercury affords a result lower than the true. This might afford some grounds for conjecturing that, although the rate of expansion evidently increases beyond the temperature of boiling water, it does not continue to increase to the end; but there is another inference from the fact, which I am rather inclined to adopt.

In calculating the temperature of melting cast-iron, from the expansion of the platinum bar, I applied a correction, upon the supposition that the same rate of increase of expansion which was exhibited by platinum between the boiling points of water and mercury continued to the higher degrees; whereas there is great reason to suppose that the rate must be an increasing one; and, although this might not sensibly affect the final result of the comparatively low temperature of melting

silver, the calculation of the temperature of melting iron, which is more than one third higher, would be sensibly affected by it. I think it therefore extremely probable that the true temperature of melting cast-iron is below 2786°.

The consistency of these results will, I trust, remove any doubts as to the competency of the pyrometer to determine fixed and comparable points of very high temperatures, and induce those connected with arts and manufactures to introduce its use, for the purpose of ascertaining many questions of the highest interest, both to practical and theoretical science. The experiments just detailed upon bars of wrought-iron removed even the only trifling objection which could be brought against its general use; namely, the expense of a platinum bar: for it is quite proved that a bar of wrought-iron is sufficient for every practical purpose, and it affords the important additional advantage of a much more open scale.

I proceed now to remark that zinc, as well as iron, appears by the Tables to present an exception to the law of an increasing rate of expansion with increasing temperature; the expansion for the 600° to boiling mercury not being so much as four times that for the 150° to boiling water. I cannot, however, from some peculiar circumstances attending the experiment, place entire confidence in the result. When, after boiling in mercury, the register was opened, the vapour was found to have gained admittance, and to have acted upon the zinc. It was firmly fixed in the cavity, and was not removed without considerable difficulty and piecemeal. At its upper end, the bar was reduced almost to a point, and was very considerably thickened at its lower end, and moulded to the bottom of the register, as if it had been partially fused. It was hard and brittle. The vapour of the mercury had probably combined with it at some temperature below the boiling point; the amalgam so formed had flowed down to the bottom of the bar, and the mercury was afterwards expelled by the boiling temperature.

I may here observe, as not unworthy of attention, that in no instance have I seen a metal acted upon by the vapour of mercury at its full boiling temperature;—even gold, which has so strong an affinity for it, comes out of it with its yellow colour perfectly unstained; but when the mercury is in the fluid form at the same temperature, the gold is immediately dissolved by it.

Under these circumstances there certainly may exist some doubt whether the full amount of expansion in zinc to the boiling point of mercury was properly registered.

On the other hand, in confirmation of the result so re-

corded, it may be seen, in Table XIV. of the expansion of the alloys, that a composition of half copper and half zinc presents the same anomaly; the expansion for the 600° to boiling mercury is not quite four times that of the 150° to boiling water. In the alloy of three fourths copper to one fourth zinc, the rate of expansion increases in a small degree; and in common brass, where the proportion of zinc is still less, it increases still more rapidly.

My purpose in instituting these experiments upon the alloys, was to observe the relation which might exist between the expansions of the pure metals and those of their mixtures; and the better to illustrate any such, I made alloys of copper with known multiple proportions of zinc and tin. I shall here present, in a tabular form, the temperatures of their melting points, as derived from their expansions to the boiling points of water and mercury; as, although I am not able to compare them with results directly obtained by immersion, we can judge, by comparison with the similar calculation of the pure metals, within what limits any error is probably confined.

Table XVI.

Fusing Points of Alloys, derived from their Expansions to 212° and 662° supposed equable.

	From 212° rate.	From 662° rate.
Brass. Copper $\frac{3}{4}$, Zinc $\frac{1}{4}$ . . .	1842°	1750°
Brass. Copper $\frac{1}{2}$, Zinc $\frac{1}{2}$ . . .	1672	1910
Bronze. Copper $\frac{15}{16}$, Tin $\frac{1}{16}$.	1761	1690
Bronze. Copper $\frac{7}{8}$, Tin $\frac{1}{8}$ . .	1773	1534
Bronze. Copper $\frac{3}{4}$, Tin $\frac{1}{4}$ . .	1755	1446
Pewter. Lead $\frac{4}{5}$, Tin $\frac{1}{5}$ . . .	403	
Type Metal. Lead and Antimony	507	

I have not included in the foregoing Table the alloy of half copper and half tin, but have exhibited its expansion to the boiling point of mercury in Table XIV. This mixture was very hard and brittle, and resembled the speculum metal of reflecting telescopes. After it had been exposed to boiling mercury, it appeared as if it had undergone partial fusion; it was set fast in the cavity of the register, and had thickened towards the lower extremity. I am inclined to think that it had nearly attained its melting point, but it was broken in removing it; and I had not an opportunity of trying any further experiment with it.

With regard to these alloys, the experiments are not nu-

merous enough to enable us to deduce with precision the general laws by which their expansions and points of fusion are governed; but enough is discernible to show that the subject is well worthy of further investigation. It appears

1st. That the expansion of the compounds is not the mean of the expansions of the simple metals of which they are composed, but bears some proportion to their relative quantities. Thus we may observe that the expansion of brass increases with the quantity of zinc which it contains, as does bronze or bell-metal with the quantity of tin.

2ndly. That the expansion of brass is in an increasing ratio to the increase of temperature till the quantity of zinc amounts to one half, when it seems to assume a decreasing rate, as we have reason to suppose is the case with pure zinc. On this account the melting points both of this mixture and zinc appear to be higher when derived from their expansions to the boiling point of mercury, than when calculated from their expansions to the boiling points of water. With this exception, there is great reason to suppose that the melting points of the alloys, from the higher rate of expansion, cannot be very far removed from the true temperatures.

3rdly. That the melting point of copper is reduced by an admixture of one fourth of zinc to nearly the average which results from the proportions of the two ingredients; but by an admixture of an equal quantity of tin it is reduced in a much greater proportion. The temperature derived from the average with zinc would be 1690°, and the corresponding temperature in the Table is 1750°. The temperature derived from the average with tin would be 1607°, but the corresponding temperature is only 1446°.

4thly. That a similar power in tin to depress the melting point of another metal is exhibited in pewter; in which we may observe that a mixture of one fifth of tin with lead reduces the melting point actually below that of either of the pure metals; and we may recall to recollection the fact, that an alloy of eight parts of bismuth, whose fusing point is 476°; five of lead, whose fusing point is 612°; and three of tin, whose fusing point is 442°,—liquefies at 212°.

I shall here subjoin a Table, in the usual form, of the progressive linear dilatation by heat of such solids as I have measured with the pyrometer to the boiling point of water, the boiling point of mercury, and their respective melting points, where they have been ascertained. I have added to their apparent expansions by the register the corresponding expansion of the black-lead; upon the assumption that the latter continues at an equal rate to temperatures above 662°; in

which it is not probable, from the preceding observations, that there is any error of material importance.

TABLE XVII.

Linear Dilatations of Solids by Heat.

Dimensions which a bar takes whose length at 62° is 1·000000.

	At 212° (150°).	At 662° (600°).	At Point of Fusion.
Black-lead ware ...	1·000244	1·000703	
Wedgwood ware...	1·000735	1·002995	
Platinum	1·000735	1·002995	(1·009926 maximum, but not fused.)
Iron (wrought)	1·000984	1·004483	(1·018378 to the fusing point of cast iron.)
Iron (cast)	1·000893	1·003943	1·016389
Gold	1·001025	1·004238	
Copper...............	1·001430	1·006347	1·024376
Silver.................	1·001626	1·006886	1·020640
Zinc....................	1·002480	1·008527	1·012621
Lead...................	1·002323	....	1·009072
Tin	1·001472	....	1·003798
Brass. Zinc $\frac{1}{4}$......	1·001787	1·007207	1·021841
Bronze. Tin $\frac{1}{4}$ ...	1·001541	1·007053	1·016336
Pewter. Tin $\frac{1}{5}$ ...	1·001696	....	1·003776
Type Metal.........	1·001696	....	1·004830

The regularity of these several expansions is very striking. As long as the metal retains the solid form, the dilatation proceeds according to a fixed law, without any sudden starts or changes; till assuming the form of a liquid it doubtless is subject to a different mode of action.

I shall conclude these observations with the results of some experiments which I made to determine, if possible, the cause of the singular change of texture in platinum, when intensely heated in the black-lead registers, which I described in my former paper*. Upon showing the bar so changed to those who were best acquainted with the working of this metal, they universally ascribed it to the action of sulphur: but nobody could explain to me why this action should require such a very intense heat; as up to the temperature of melting cast-iron, to which it had several times been exposed, no change took place; but the bar remained perfectly soft and malleable.

In De Ferussac's Bulletin for November 1830, there is an abstract of my paper on the Pyrometer, which the Editor concludes with the observation, that "unfortunately I inclosed in the crucible which contained the register and the bar of platinum some pieces of iron, without being aware of the fact, which is known to all the workmen who manufacture pla-

* See Phil. Mag. and Annals, vol. x. pp. 354, 355.—EDIT.

tinum, that the mere presence of iron is enough to communicate brittleness to that metal."

Upon inquiry amongst workmen in this country I cannot find that such a property has ever been observed in the course of their experience; and when I consider that the bar in the cavity of the register was perfectly preserved from contact with the iron nails; and moreover, that it had actually been plunged into melted iron without any change of properties; I cannot suppose that the alteration depended in any way upon this circumstance.

To resolve these doubts I took 116 grains of the brittle platinum, which had been ground without difficulty to a fine powder in a steel mortar, and boiled them in nitro-muriatic acid till I had effected a complete solution;—a little of this solution produced a scarcely perceptible cloudiness in a solution of muriate of baryta. This I have reason to think was owing to a slight impurity in the acids employed; I infer therefore that there was no sulphur in the metal. I proceeded to evaporate the solution; which towards the end of the process assumed a gelatinous appearance. When in this state, I poured alcohol upon it; and as the acid still remained in excess, a violent reaction took place with extrication of nitrous gas. I then evaporated to dryness and continued the heat; till the salt of platinum kindled spontaneously, and finally was left in a spongy state. This was again digested in nitro-muriatic acid, and the solution carefully evaporated to dryness. The muriate of platinum was then dissolved in water, and a sandy residue remained; which, when well washed and heated to redness, was of a grayish-white colour, and had all the properties of silica: it weighed 3·5 grains. There can therefore, I think, be little doubt that at the high temperature to which it was exposed, platinum took up as much as 3 per cent. of silica; or, more probably, a quantity of its base equivalent to that quantity of the earth, to which it owed all its change of character and properties. A temperature considerably above that of melting cast-iron appears to be necessary to this combination; which is analogous in many respects to the absorption of carbon by iron in the process of making steel by cementation*.

* The combination of platinum and the base of silica formed by Mr. Daniell, had before been noticed by Descotils and Chenevix; but they considered it to be a carburet of platinum. Boussingault, however, re-examined it, and found it to be in reality a compound of the base of silica with that metal. Descotils and Chenevix obtained it by heating platinum with charcoal, as Mr. Daniell has done by heating platinum with black-lead ware. Boussingault found that when the metal was heated with lamp-black this combination was not formed.—See Thomson's Inorg. Chem. vol. i. p. 665.—Edit.

L. *Notes on the History of English Geology.* *By* William Henry Fitton, *M.D. F.R.S. &c.*

[Continued from p. 160.]

BUT the most important observations, perhaps, that have ever yet appeared on the subject of stratification, are those of the Rev. John Michell, in a paper 'On the Cause and Phenomena of Earthquakes,' published in the Philosophical Transactions for 1760*; where the author not only describes the general appearance and structure of stratified countries, but explains most clearly the arrangement of the strata in England:—and this, not as confined to Britain, but as exemplifying a general principle, which he supposes to hold universally in other parts of the globe.

'The earth,' he says, '(as far as we can judge from the 'appearances,) is not composed of heaps of matter casually 'thrown together, but of regular and uniform strata. These 'strata, though they frequently do not exceed a few feet, or 'perhaps a few inches in thickness, yet often extend in length 'and breadth for many miles, and this without varying their 'thickness considerably. The same stratum also preserves a 'uniform character throughout, though the strata immediately 'next to each other are often totally different.'

The perpendicular fissures of the strata are then noticed, their bendings, and their position, which is stated to be, in a general view, horizontal.—'What is very remarkable, however, 'in their situation is, that from most, if not all, large tracts of 'high and mountainous countries, the strata lie in a situation 'more inclined to the horizon than the country itself, the 'mountainous countries being generally, if not always, formed 'out of the lower strata of earth. This situation of the strata 'may be not unaptly represented in the following manner: 'Let a number of leaves of paper, of several different sorts 'of colours, be pasted upon one another; then bending them 'up together into a ridge in the middle, conceive them to be 'reduced again to a level surface by a plane, so passing 'through them as to cut off all the part that had been raised; 'let the middle now be again raised a little, and this will be 'a good general representation of most, if not of all, large

* Vol. li. Part ii. Sections 37 to 49, p. 566, &c.—Mr. Farey states that Mr. Michell was appointed Woodwardian Professor at Cambridge, about 1762; an office which he held, we believe, for about eight years. He was then, unfortunately for Geology, transferred to the Rectory of Thornhill, near Wakefield, in Yorkshire; and died on the 21st of April 1793. Mr. Michell was the author also of some excellent Astronomical papers in the Philosophical Transactions.

'tracts of mountainous countries, together with the parts ad-'jacent, throughout the whole world*.

'*From this formation of the earth, it will follow, that we 'ought to meet with the same kinds of earths, stones, and mi-'nerals, appearing at the surface, in long narrow slips, and 'lying parallel to the greatest rise of any long ridges of moun-'tains; and so, in fact, we find them.* The Andes, in South 'America, has a chain of volcanos that extend in length above '5000 miles: these volcanos, in all probability, are all de-'rived from the same stratum. Parallel to the Andes is the 'Sierra, another long ridge of mountains, that run between 'the Andes and the sea:' and 'these two ridges of moun-'tains run within sight of one another, and almost equally: 'for above a thousand leagues together † being each at a me-'dium above twenty leagues wide.

'The same thing is found to obtain in North America also. 'The great lakes, which give rise to the river St. Lawrence, 'are kept up by a long ridge of mountains, that run nearly 'parallel to the eastern coast. In descending from these to-'wards the sea, the same sets of strata, and in the same order, 'are generally met with throughout the greatest part of their 'length‡.

'*In Great Britain we have another instance to the same pur-'pose, where the direction of the ridge varies about a point from 'due north and south, lying nearly from N. by E. to S. by W.*§ 'There are many more instances of this to be met with in the 'world, if we may judge from circumstances, which make 'it highly probable that it obtains in a great number of places; 'and in several they seem to put it almost out of doubt.

'The reader is not to suppose, however, that, in any in-'stances, the highest rise of the ridge, and the inclination 'of the strata from thence to the countries on each side, is 'perfectly uniform, for they have frequently very considerable 'inequalities, and these inequalities are sometimes so great 'that the strata are bent, for some small distance, even the 'contrary way from the general inclination of them. *This 'often makes it difficult to trace the appearance I have been re-'lating, which, without a general knowledge of the fossil bodies 'of a large tract of country, it is hardly possible to do.*

'At considerable distances from large ridges of mountains, 'the strata, for the most part, assume a situation nearly level;

* 'Fig. 3. (Plate II.) represents a section of a set of strata, lying in the 'situation just described. The section is supposed to be made at right 'angles to the length of the ridge, and perpendicular to the horizon.'

† 'See Acosta's Natural History of the Indies.'

‡ 'See Lewis Evans's Map, and Account of North America.'

§ 'Of this,' Mr. Michell adds in a note, 'I could give many undoubted proofs, if it would not too far exceed the limits of my present design.'

'*and as the mountainous countries are generally formed out* '*of the lower strata, so the more level countries are generally* '*formed out of the upper strata of the earth.*

'*Hence it comes to pass that, in countries of this kind, the* '*same strata are found to extend themselves a great way, as* '*well in breadth as in length.* We have an instance of this in 'the chalky and flinty countries of England and France, which '(excepting the interruption of the Channel, and the clays, 'sands, &c. of a few counties,) compose a tract of about three 'hundred miles each way.'

The account of the districts in America, above referred to, has been confirmed, we believe, in general, by more recent observations: and nothing can be more clear than Mr. Michell's exposition of the principle of the stratification of England. That he was acquainted with the detail also, is proved by a memorandum discovered in 1810, among the papers of Mr. Smeaton, then in the hands of Sir Joseph Banks; in which are enumerated several of the principal beds, from the chalk down to the coal; detached portions, several miles distant from each other, being, in two instances, associated under the same name.—This paper is as follows*:

'*Mr. Michell's Account of the South of England Strata.*

	Yards of Thickness.	*Present Names.*
'Chalk.	120	Chalk.
'Golt.	50	Gault.
'Sand of Bedfordshire.	10 to 20	Woburn Sands, — [Lower Green-sand.]
'Northamptonshire lime, and Portland lime—lying in several strata.	100	Portland and other Oolites.
'Lyas strata.	70 to 100	Lias.
'Sand of Newark. . . . . about	30	?
'Red clay of Tuxford, and several.	100	New-red-sand-stone.
'Sherwood Forest, pebbles and gravel.	50 unequal	Probably superficial Gravel.
'Very fine white sand.	uncertain	?
'Roche Abbey and Brotherton limes.	100	[Magnesian Lime-stone.]
'Coal strata of Yorkshire.'		Coal-measures?

It is extraordinary that the very remarkable paper in the Philosophical Transactions from which the foregoing extracts

* We are indebted to the late Mr. Farey for the publication of this valuable document, in the Philosophical Magazine, vol. xxxvi. p. 102, &c.; and the list of modern names above given has been adopted from him. The *thickness* of most of the strata, he justly observes, is greatly underrated. —The list was found, in Mr. Smeaton's writing, on a part of the back of a letter bearing the London post-mark of November 21, 1788. Smeaton himself died in September 1792.

have been taken, embracing general principles of such importance, does not appear to have been mentioned, or alluded to by any writer on geology, either in this country or upon the Continent, during a subsequent period of more than fifty years. This may, perhaps, be accounted for, in some degree, by the title and immediate subject of the paper itself; but it must be ascribed principally to the very languid state of inquiry as to the structure of the earth, in England, for a long time after its appearance*.

[A still more interesting question is,—Whether by the words "*fossil bodies*," without a general knowledge of which 'in a large tract of country,' Mr. Michell states, 'it is hardly 'possible to trace the appearances he has been relating'—he intended to signify the *organized remains* included in the strata:—For, if that were his meaning, there would really be very little in the doctrines of modern geology, in which, as to principle, he did not take the lead. This, however, does not appear to have been the case. Mr. Sedgwick has very justly stated†, that no part of the Woodwardian Collection, which was for some years under Mr. Michell's immediate superintendance is *stratigraphically* arranged; and that, not only in the works and catalogues of Woodward, but in the language of other English naturalists of the last century, every *mineral* substance was designated under the general term '*fossil;*' organic remains almost always distinguished by the name of '*extraneous* fossils, *organic* fossils,' &c. Nor is there any reason to suppose, that Mr. Michell's arrangement of the British strata was made public till the accidental discovery of the slight document above mentioned, many years after Mr. Smith's inquiries had begun; indeed, at a period when his Map of England was far advanced towards publication.]

The next author of note is WHITEHURST, whose *Inquiry into the Original State and Formation of the Earth* was first published in the year 1778, and reprinted, with considerable improvements, in 1786. A great part of this book is infected with that taste for cosmogony which had misled so many of the author's predecessors; but if the reader be not repelled by the formidable chapters '*Of the component parts of chaos whether homogeneous or heterogeneous*,' and '*Of the period of human life before and after the Flood*,' he will find some excellent remarks upon organized fossils; and in the latter part

* After the first publication of Dr. Fitton's article in the Edinburgh Review, Mr. Michell's paper on Earthquakes was reprinted in full, in Phil. Mag. vol. lii. beginning at p. 186.—EDIT.

† Address to the Geological Society, at the Anniversary, February 1831. Proceedings, p. 274 (or Phil. Mag. and Annals, N.S. vol. ix. p. 275.—EDIT.)

of the volume, especially the chapter 'on the Structure of Derbyshire and other parts of England,' abundant proofs of the author's acuteness and fidelity as an observer. His statements, indeed, concur precisely with those of Mr. Michell; 'the arrangement of the strata being such,' he tells us, 'that 'they invariably follow each other, as it were, in alphabetical 'order, or as a series of numbers. *I do not mean to insinuate 'that the strata are alike in all the different regions of the 'earth, with respect to thickness or quality—for experience shows 'the contrary;* but that *in each particular part*, how much so'ever they may differ, yet they follow each other in a regular 'succession*.'—'It was my intention,' he says in another place, 'to have deposited specimens of each stratum, with its pro'ductions, in the British Museum, arranged in the same order 'above each other as they are in the earth; being persuaded 'that such a plan would convey a more perfect idea of sub'terraneous geography, and of the various bodies inclosed in 'the earth, than words or lines can possibly express †.' But it is remarkable that Whitehurst, at the close of his work, appears to dwell with much more pleasure on that part which relates to the early ages of the world, and the condition of its antediluvian inhabitants, 'who slept away their time in sweet 'repose upon the ever verdant turf,' than upon the truly important and substantial part of his performance.

The most direct instance that we have met with, of the actual tracing the course of any of the strata in England, before the commencement of Mr. Smith's investigations, occurs in the celebrated work of SMEATON on the Eddystone Lighthouse ‡; and it affords an excellent proof of the practical benefit to be derived from geological inquiries. Mr. Smeaton was in want of lime which possessed the property of forming a good cement for works exposed to the sea; and finding the lime afforded by the lias limestone at Aberthaw, on the coast of Glamorganshire, to answer his purpose §, he was led to seek for stone of the same qualities in other places. This he found, in the first instance at Watchet, on the Somersetshire coast, 'where 'all agreed, that they were the very same stratum of lias lime-

* Whitehurst, Second Edition, pp. 178, 179.

† Pages 204, 205.—This project has since been executed; Government having, in 1806, purchased, for the British Museum, Mr. Smith's collection of fossils, arranged according to the order of the strata:—an acquisition certainly of the highest interest in the scientific annals of our country, and deserving a most distinguished place in a great national repository.

‡ London, folio, 1791. Sections 168–190, &c. In the Introduction it is stated that the book was printed in 1786.

§ The best cement was found to be a compound of equal parts of blue-lias lime and puzzolano.

'stone, that were found on each side the Channel, though at 'the distance of twenty miles.' He went accordingly, to Watchet, and examined the situation of the beds there, which he has very well described; and he subsequently traced the progress of the lias, through Monmouthshire and the intermediate counties, as far north as Newark in Nottinghamshire; a course which corresponds precisely with the results of more recent investigation. He mentions likewise, that Mr. Cavendish and Dr. Blagden had assured him of its existence at Lyme, on the coast of Dorsetshire; which is the more remarkable, as a considerable mass of other strata intervenes, upon the surface, between that place and those which Mr. Smeaton had examined himself. It is not however improbable, that Smeaton's inquiries upon this subject may have been connected with some previous communication with Mr. Michell; since he appears to have received from that gentleman, the list of the strata to which we have already referred, before the publication of his own work on the Lighthouse.

It is difficult to trace the history of WERNER's doctrines; the most important of his tenets having been delivered only in the form of lectures; while the writings of his pupils, who confessedly borrowed from their master, are generally diluted with large additions of their own. In England especially, a correct view of Werner's geological system was not obtained till long after its promulgation: it was not indeed accessible to persons unacquainted with the German language, till the publication of Mr. Jameson's volume of Geognosy, in 1808; and was very imperfectly appreciated for a considerable time afterwards; the controversy between the Wernerian and Huttonian schools, having called off the attention of those engaged in the study of Geology, to the speculative department of their subject, from the more solid occupation of inquiry into the actual structure of the globe. The *Kürze Klassifikation* of Werner, a brief but valuable arrangement and description of rocks, published by himself in 1787*, has no allusion nor hint at the doctrine of *Formations*, the term not once occurring in that work. Nor was the distinction of the *transition* from the flœtz class introduced into his arrangement for some years afterwards; grey-wacké being placed, in the list of 1787, among the *flœtz* sand-stones. The opinions of Werner, as to the origin of the basaltic rocks, were formed after his examination of the Scheibenberg in 1787†. The

* *Kürze Klassifikation und Beschreibung den verschiedenen Gebirgsarten.* Von A. G. Werner, &c. Dresden, 1787. 4to, pp. 28.

† Bergmännisches *Journal*, 1788, vol. ii. p. 845.

doctrine of formations was delivered in his lectures only, and may be dated as of 1790 or 1791; that of the *transition-class* not until 1795 or 1796. But his theoretic views, as to the deposition of rocks in general, and the configuration of the earth's surface,—which, after all, (if what relates to the overlying formations be excepted,) are little more than a selection from the doctrines of preceding writers,—may be collected from his work on Veins, first published in November 1791; at which time it is certain that he was acquainted with the works of Whitehurst, for they are quoted in the book last mentioned.

The true merit of Werner, on which it is probable his reputation as a naturalist will ultimately rest, appears to consist, in his having drawn the attention of geologists, explicitly, to the *Order of succession* which the various natural groups of rocks are found in general to present; and in having himself developed that order, to a certain extent, with a degree of accuracy which before was scarcely attainable, from the want of sufficient methods of discriminating minerals and their compounds. He was, we believe, the first to observe, or the first to diffuse the doctrine, that the masses or strata, constituting the surface of the globe, present themselves *in groups* or assemblages, the members of which are generally associated wherever they occur, and are so connected as to exhibit a certain unity of character. To such assemblages Werner gave the name of *Formations;* and his doctrine (or hypothesis, if this latter term be preferred,) was, that the exterior of the earth consists of a series of these formations, laid over each other in a certain determinate order. Not that the whole series is anywhere complete; but that the relative place of its members is never departed from. Thus in the ascending series A, B, C, D, it may happen that B or C, or both, may be occasionally wanting, and consequently D be found immediately above A; but the *succession* is never violated, nor the order inverted, by the discovery of A above the formations B, or C, or D, nor of B above those that follow it, &c.*

A very important exception, however, to this regularity of arrangement, is found in the position of that great class of compound rocks, which includes all those of the trap family, the porphyries, syenites, and some at least of the granites of Werner. The compounds of this tribe, in general, agree, not only in possessing the characters of crystallization, and in being wholly destitute of organic remains, but in exhibiting, at their junction with the stratified substances, the

[* The substance of this and the following paragraphs, is taken from a preceding article in the Edinburgh Review, by the author of the present paper; Vol. xxix. Nov. 1817, p. 71.]

most obvious marks of violent derangement; and the trap rocks, in the form of large and numerous veins, are found to traverse, indiscriminately, all the other formations. It is impossible, then, to believe, that the same laws have governed the disposition, both of these compounds, and of the strata which contain organic remains, and exhibit greater uniformity of structure; and every arrangement which assigns to both a common origin, or attempts to include the trap, and other similar formations, in the general series of rocks, must be defective, and radically inconsistent. The capital mistake of Werner (to which he was led, no doubt, by an erroneous theory), was, that he attempted such a combination, and neglected those demonstrations of violence and disturbance.

In England, although the greater part of the country wants the more striking features of the primitive tracts, it fortunately happens that the series of secondary strata is nearly complete; and, when our great extent of coast is taken into the account, few countries present a field for geological observation in which the phænomena are at once so varied and so well displayed. It will soon be perceived that the inferences from Mr. Smith's examination of this country, coincide, to a great extent, with those of Werner: and this coincidence, between the results obtained by two independent observers, through channels of inquiry so different, is no small confirmation, both of the fidelity of their observations, and of the correctness of their deductions from them.

[To be continued.]

LI. Observationes quædam ad NARCISSINEÄS spectantes; *Autore* A. H. HAWORTH, *Soc. Linn. Lond.—Soc. Horticult. Lond.—Soc. Cæs. Nat. Curios. Mosc.—Soc. Reg. Horticult. Belgic.—necnon Bot. Reg. Ratisb., Socius : &c. &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN this my thirty-second communication to your valuable and scientific Journal, a few material alterations and amendments, together with some novelties towards the improvement of my *Narcissineärum Monographia*, may be acceptable to your readers; made from the living plants during the blooming season of the fine spring of 1832, during great part of which time the fragrant *Narcissineæ* were very ornamental, and I think finer than, up to that time, I ever beheld them. But there are nevertheless many dubious points I am not even yet able completely to clear up respecting these intricate

plants; so that the motto of the Monograph remains in force,—

"Multum adhuc restat, multumque restabit."

At the end I have added, by way of postscript, a few other amendments and observations on kindred bulbous plants; and remain, Gentlemen, yours, &c.

A. H. HAWORTH.

Observationes quædam ad NARCISSINËAS *spectantes.*

Obs. In the generic character of *Corbularia* the seeds are quadrifariously, rather than bifariously inserted as stated in *Narcis. Monog. ed.* 2.; but I have not yet been able to examine more than one species.

Obs. To *Corbularia Bulbocodium* add, as synonyma, *Lob. Ic.* 119. 2.—*Bauh. Hist.* 2. 559. *f.* 2.

Obs. To *Corbularia lobulata* add: *Narcissus tenuifolius Redout. Lill.* 486.

Obs. To *Corbularia serotina* add, Affinium *Folia*, longiora et duplò validiora, decumbenti-humifusa, valdè flexuosa, vel quasi serpentina. Ex eodem bulbo *scapi* subindè tres, si optimè culto.—Query: Is *Narcissus infundibulum* of Lamarck a species of *Corbularia*.

Obs. In the genus *Ajax* the seeds are from 2- to 4-fariously inserted.

Obs. *Ajax minor* of *Narc. Monog.* is *N. minor Redout. Lill. t.* 480.

maximus. Ajax. (The largest yellow): *Obs.*—*Folia* latiora quàm in *A. majore*, minùs torta. *Corollæ* laciniæ magìs expansæ, seu ferè horizontales. *Corona* prægrandis patens, lobis longè indistinctioribus vel potiùs plicatìm multi- et magni-serratis, serrisque ad oras reflexis. *Flos* paullò saturatior flavus, ferè 3 uncias longus, et 4 unc. latus. In *A. majore*, *corollæ* laciniæ semierectæ, lobis subrecurvis. *Flos* $3\frac{1}{2}$ unc. lat., $2\frac{1}{2}$ long.

anceps. A. (light green-leaved): corollæ laciniis albis, coronâ lætè luteâ, foliis loratis obtusis latiusculis lætè viridibus, scapo compresso ancipiti.

Ajax lorifolius β. *Narcis. Monog. ed.* 2. *p.* 8.

Florebat in horto amici Dom. Sweet, Aprili mense A.D. 1832, post *A. lorifolium Nob.* Distinguitur optimè ab affinibus proximis, lætè viridibus foliis, florum albis laciniis, scapoque valdè compresso. Ante *Ajacem bicolorem* certè locarem, propter albas lacinias, tempusque florendi; sed satìs ab eo in foliis angustioribus et non glaucis differt.

propinquus β. A. (The paler yellow): *Ajacem Telamoneum* simulat, sed laciniæ longissimè saturatiores cum coronâ latiore et breviore similitèr luteâ, lobis plùs duplò majoribus. Affinior forsàn *Ajace majori*, à quo differt luteo flore, (nec aurantio-luteo seu flavo,) duplòque minore, coronâ minùs expansâ lobis minùs circularibus, et *foliis* minùs flexuoso-tortis.

Obs.—Stigma antheras, in nostro solitario exemplo, subhumilius, qui non est in *A. maximo*, *majore*, *telamonio*, vel *propinquo* α. sed cum his omnibus habet magnam affinitatem. *Flos* 2 unc.; 4 lin. long., 3 unc. lat.

Observationes ulteriores.—Laciniæ ovato-lanceolatæ semiexpansæ lætè luteæ variè tortulæ *coronam* breviores. *Corona* paullò magìs lutea plicatula, ore parùm expanso altè et distinctè 6-lobato, lobis magnis subsemuncialibus plusquàm semicircularibus præplicatis et obsoletè irregularitèr crenulatis.

Obs. Communicavit amicus Dom. Penny, A.D. 1832. Fortassè propria species.

Obs.—Illus, *Haw. Narciss. Monog. ed.* 2. *p.* 10.

triandrus. I. (The snowy white-flowered): 1—5-florus: corollæ niveæ, laciniis lanceolatis coronâ integrâ duplò longioribus, stylo exserto.

Narcissus triandrus *Linn. Sp. Pl.* 416. 9.—Narcissus juncifolius flore albo reflexo, *Park. Par. t.* 93. *f.* 2.

Obs. Corolla tota nivea. *Linn. l. c.*—"Bearing out of a skinny husk three or four or more snow-white flowers." *Park. l. c.*—*Swertius Florileg. t.* 29. *f.* 4? Fortè species major minùs reflexa, stylo incluso.

cernuus. I. (The pale yellow): corollæ ochroleucæ laciniis planis lanceolatis oris albis coronâ integrâ saturatiore sesquilongioribus, stylo incluso, scapo teretiusculo.

Ganymedes cernuus *Salisb. Prod.* 223.—*Haw. Narciss. Monog. ed.* 2. *p.* 10, 1. Narcissus triandrus *Bot. Mag.* 48. *nec Linn.*

N. coronatus *Sch. Syst. Veg.* 7. *p.* 986.—*Swertius Florileg. t.* 65. *f.* 7.

β. Stylo excluso. An idem?

albus. I. (The tortuose white): corollæ albæ laciniis lanceolato-linearibus tortis coronâ integrâ duplò longioribus, stylo longè exserto, scapo compresso.

Ganymedes albus *Haw. Narciss. Revis. in Pl. Succ. p.* 208.—Illus triandrus *Haw. Narciss. Monog. ed.* 2. *p.* 10. *nec Linnæi* N. triandrus.

Obs. N. Coornei. *Red. Lill. v.* 8. *ad finem*, *et Rudb.*

Elys. t. 65. *f.* 2.—*Moris.* 8. *p.* 4. *t.* 23. *fig. penult. addito bulbo.*—*Chabr. Sciagr.* 217. *f.* 2. *Lob. adv. alt.* 498. *ic.*—*Bauh. Hist.* 2. 599 fortassè est imperfecta et casualis (ex malâ culturâ vel transplantatione) varietas hujus speciei, et indè imperfecta, redacta, quoque tortiles flores.

orientalis. SCHIZANTHES, *Narciss. Monog. ed.* 2. *p.* 12.

Obs. Varietatem flore pleno sive semipleno vidi crescentem in horto Dom. Young, apud Epsomam, Aprili mense A.D. 1832.

major. JONQUILLA. Variat fl. pleno.
media. JONQUILLA. Variat fl. pleno.
minor. JONQUILLA. Variat fl. pleno.

Genus HERMIONE. *Sectio* GRANDIFLORÆ.

solaris. HERMIONE, *Narciss. Monog. ed.* 2. *p.* 15. Est solùm planta debilis sive immatura, vel ex malâ culturâ, *Herm. cupularis,* corollis paucis, cum laciniis depauperatis omninò angustioribus et distinctioribus quàm in *H. cupulari:* propterea e systemate expurgandam est.

grandiflora. H. (great orange-bordered): sub-6-flora: corollæ amplissimæ rotularis laciniis albis, coronâ patulâ plicatìm crenulatâ primò supernè aurantiacâ, demùm totâ luteâ, foliis glauculis.

H. grandiflora *Synops. Succ. app. p.* 330.—Narciss. Trewianus *Bot. Mag. t.* 940. (*castigandis synonymis*), medio ætati floris representatus.—H. Trewianus *Narciss. Monog. p.* 16, *excluso synonymo Trewii et Sweetii.*

β. fissicorona (The cloven-cupped): subtriflora: coronâ majore patenti trilobatìm fissâ lobis magnis 1—4-relobulatis truncatis.

flexiflora. H. (great yellow-cupped) 3—8-flora: corollæ amplissimæ rotularis laciniis horizontalibus albis primò planis mox variè flexis, coronâ patulâ plicatìm crenulatâ semper totâ luteâ, foliis glauculis.

H. flexiflora *Narciss. Monog. p.* 16.—Bazleman major *Trew. Fl. Imag. Narciss.* 1. *t.* 23. *H. Trewiana. Sw. Fl. Gard.* 118.

subcrenata. H. (middle yellow-cupped) 3—8-flora: corollæ laciniis subreflectentibus albis, coronâ patulâ plicatulatìm crenulatâ luteâ 3-plò longioribus, foliis loratis angustioribus glauculis.

H. subcrenata *Narciss. Monog. ed.* 2. *p.* 16. Bazleman minor *Trew. Fl. Imag. Narciss.* 2. *t.* 60.

Obs. Coronæ ipse margo subindè in primâ florescen-

tiâ, cum aurantiaco aliquantillùm gaudet, citiùs evanescente. Prioribus duabus omninò minor.

crenulata. H. (lesser saffron-bordered) 3—8-flora: corollæ laciniis subreflexis albis, coronâ patulâ luteâ croceo altè marginatâ plicatulatìm crenulatâ 2—3-plò longioribus, foliis loratis angustioribus glauculis.

H. crenulata *Narciss. Monog. p.* 16, *excluso synonymo Trewii*, quod præcedentem pertinet.

viridifolia. H. (narrow green-leaved) 2—3-flora: corollæ laciniis semireflexis albis, coronâ parvâ patulâ luteâ, croceo primò paululùm marginatâ plicatulatìm crenulatâ 2—3-plò longioribus, foliis lineari-loratis præangustis supernè debilitèr variè flexuoso-recurvis viridibus.

Nova species, quàm prioribus duabus bulbo et floribus minor, foliis longè angustioribus, et certè viridioribus sesquipedalibus 5 lin. lat. concavo-inflexis, subtùs convexis striatis. *Florebat* in hortulo nostro cum duabus ultimis, ubi per multos annos colui.

Sectio nova: REFLEXIFORÆ.

Corollæ laciniis plusquàm semireflexis, reflexisve.

reflexa. H. (white reflexed) sub-5-flora: corollæ secundæ laciniis semi-plùsve-reflexis ovatis imbricatis variè flexulis, coronâ cupulari ore contracto aurantiaco subduplò longioribus.

Nova species, ultimâ ferè duplò minor. Communicavit amicus Dom. Penny.

Habitat in Europâ australiori.

Floret Aprili.

Obs. Folia lorato-attenuata pedalia sub-semunciam lata, carinata glaucescentia. *Scapus* ancipite-teres striis elevatis, foliis cavo-canaliculatis florendi temp. paullò altior. *Pedunculi* graciles 3-angulares elevato-striatuli. *Corollæ* laciniæ demùm horizontales. *Corona* integra vel subindè subundulata. *Antheræ* rubro-aurantiacæ, 3, extra tubum exsertæ.

neglecta. H. (slender white reflexed) sub-4-flora: corollæ laciniis lineari-lanceolatis reflexis distinctis niveis, coronâ subcupulari luteâ 3—4-plò longioribus, foliis mediocribus erectis lorato-attenuatis carinulatis.

Obs. Sub nomine *Narcissi neglecti*, ex Neapoli accepit amicus Dom. Rob. Sweet, et in ejus horto florebat A.D. 1832, Aprili mense.

Obs. Quàm priore gracilior, glaucior. *Corollæ* laci-

niæ magìs reflexæ graciliores. *Corona* brevior ore non contracto. *Antheræ* luteæ nec aurantiacæ, 3 extra tubum ut in eâ. *Folia* 11 unc. long. florendi temp. et *scapo* breviora.

Subsectio, coronâ acetabuliformi.

craterina. H. (open-cupped yellow) sub-7-flora: corollæ laciniis luteis subincurvulis, coronâ perluteâ crateriformi ore subrecto, plùs duplò longioribus.

Nova species. Patriam nescio. *Florebat* medio Aprilis A.D. 1832 in hortulo meo.

Obs. Floris tubus luteus, basi virescens, laciniis longior. *Folia* florendi temp. vix semunciam lata ensiformi-lorata, supernè sæpè flaccidè recurvula glaucula striatula, inter breviora et planiora. *Scapus* vix compressus teretiusculus striatulus glaucus.

Pone *H. aperticoronam* mihi, locarem. Per multos annos colui.

Sectio CITROCORONÆ.

Obs.—Hermione floribunda Narciss. Monog. ed. 2. *p.* 17. was a Dutch specimen given me by Mr. Sabine, from the Chiswick Garden, by the name of "The Grand Monarque," and is well preserved in my Herbarium, with 16 flowers in one spathe. And I have this year seen it from Holland, in Mr. Dennis's fine collection in his Nursery in the King's Road, Chelsea, also with 16 flowers on one scape, but named "The Grand Primo Citronière," which I am assured is its proper florist's name. It has perhaps the broadest leaves of all the genus, measuring when adult, in my garden, 2½ feet long, and 1½ inch broad, on a plant which had only 14 flowers in one spathe. Mr. Dennis had likewise The Grand Monarque from Holland; and he has seen 18 flowers on its scape, but their laciniæ are far more acute, and may be characterized in the following manner:

acuminata. H. (The acute-flowered Nosegay) 8—18-flora: corollæ magnæ laciniis albis sæpè productìm ovato-acuminatis subreflexo-incurvulis, coronâ amplâ citrinâ suberectâ integrâ 3—4-plòlon gioribus.

The Grand Monarque, Hortulanorum.

Obs. Cætera cum *H. floribundâ Nob.* (The Grand Primo Citronière of the Dutch) concordat, sed sæpiùs, non semper, minor foliis minoribus floribus sæpè paucioribus.

Along with these fine-grown plants at Mr. Dennis's I saw an *Hermione* with 23 flowers on one scape; and another with a proliferous *spathe*, from the centre of which arose a sort of second *scape* above the primary flowers, bearing 4 peduncles, each having an open flower upon it. I never saw a proliferous scape before in this genus; but it is represented, and recopied in several of the works of the elder botanists. I likewise saw at the same time *Hermione deflexicaulis*, and others, with 21 flowers; and *H. polyantha Loisel.* with 19 flowers on separate scapes each. In my own garden, several scapes also bore 21 flowers; *Queltia aurantia* bore 2 flowers in one spathe, which I have seen only once before: and *Corbularia serotina Nob.* in Mr. Sweet's garden bore 2 flowers on one scape; and a monstrous union, or casual coalescence of *two such scapes* bearing together 4 flowers, I have examined in the Herbarium of the late Sir James Smith.

decora. H. *Narciss. Monog. ed.* 2. *p.* 17.

Obs. Under this species I now place the following var., rather than where it stands in my Monograph. My plant bore 9 flowers in a head, whose segments were about twice as long as their cup.

β. major, foliis adultis $1\frac{1}{4}$ unc. latis, floribus præcocioribus. *H. polyantha β. Narciss. Monog. p.* 18.

polyantha. H. *Loisel.*: 3—20-flora: coronâ sulphurascente subcrateriformi plicatulatìm crenulatâ mox albâ, corollæ laciniis ovatis niveis sesquiduplò breviore, foliis prælatis perviridibus planiusculis.

Obs. Bulbus inter maximos. Varietates belgicæ plures in hortis coluntur.

Folia inter erectiores, sæpè unciam lata.

Luna. H. 3—4- rariùs 5-flora: corollæ laciniis niveis imbricatis, coronâ primò pallidissimè citrinâ mox albâ sesquiduplò longioribus, foliis ensiformi-loratis perviridibus planis.

H. Luna *Nob. Narciss. Revis. p.* 143.

Obs. Folia sæpè 8 lin. lata supernè debilitèr flexuoso-recurva. *Flores* et bulbus minores quàm in præcedente.

Sectio ANCIPITES.

chrysantha. H. *Narciss. Monog. ed.* 2. *p.* 18.

Obs. The plant described in the place cited was in

a weak state, and has since produced 5 flowers in a spathe, with a less compressed scape, and the floral segments only 3 or 4 times longer than the crown: wherefore it should be removed from this section, and placed in, and arranged at the head of, the *Section* FLAVIFLORÆ.

Its presumed Italian origin is given up.

dubia. H. (The lesser white): scapo sub-9-floro ancipiti, corollæ laciniis brevibus ovatis incurvis imbricatis horizontalibus tubo brevioribus coronâque cyathiformi undulatìm subintegrâ subtriplò longioribus.

Narcissus dubius *Gouan et Willd.* Herm. dubia *Narciss. Monog. p.* 19.

Communicavit florentem amicus Dom. Penny, Apr. 10, A.D. 1832.

Obs. Scapus valdè compressus et acutè anceps, grossè vel subangulatìm striatus virescens, humilior quàm *H. jasmineâ*, et tota planta longè minor. *Folia* subvirescentia ensiformi-lorata, paululùm torta carinata obtusiuscula, basin versus intùs inflexo-canaliculata, *scapo* florendi temp. parùm altiora, dodrantalia, et 7 lineas lata. *Spatha* albida ad oras quasi exusta. *Pedunculi* breves subunciales angulatìm subsemicylindrici. *Flores* nivei concolores unciam lati et confertìm quasi imbricati. *Tubus* basi virescens obtusè angulatus. *Genitalia* nivea, exceptis solùm aurantiacis polliniferis *antheris*, harum 3, extra *tubum*, sed *stigma* trilobum humiliores.

jasminea. H. (The jasmine-like) sub-5—9-flora: corollæ elegantissimæ niveæ laciniis lanceolatis stellatis basi subimbricantibus, coronâ erosulâ sub-5-plò longioribus.

Hermione papyratia β. jasminea *Nob. in Narciss. Revis. p.* 143. A.D. 1819. et H. jasminea *Nob. in Phil. Mag.* 1830, *p.* 133, et *Narciss. Monog. ed.* 2. *p.* 19. —Narcissus niveus *Loisel., et Schultes Syst. Veg. v.* 7. *p.* 976.

Obs. I believe these synonyma to be correct, but have never been able to procure a sight of the work of Loiseleur, and cannot at present determine which is the elder name. All these pure white-flowered plants were well known to and figured by the elder botanists; but I cannot take up the valuable space in the pages of the Philosophical Magazine, which it would require, to unravel their synonyma.

trifida. HERMIONE. sub-3-flora: corollæ laciniis subochroleucis coronâ subaurantiacâ cupulari erectâ, seu subcampanulatâ trifidâ, (lobis bifidis,) 2—3-plò longioribus, foliis loratis 9 lineas latis planioribus obtusis glaucis.

Genus NARCISSUS *Linn., et Nob. Narciss. Monog. ed.* 2. *p.* 20.

brevitubatus. NARCISSUS. (The short-tubed small-crown): corollæ laciniis niveis præimbricatis ovatis logitudinalitèr subplicatulis tubo subduplò longioribus, coronâ erectâ parvâ intensè luteâ croceo tenuissimè cinctâ.

Obs. Corollæ laciniæ basi ad oras demùm reflexæ, sed supernè variè flexæ et ad oras inflexæ.

Obs. This description was made from a single living specimen, without leaves, which was grown in Mrs. Marriot's rich garden on Wimbledon Common, and given to me, at the end of April 1832, by my friend Mr. E. D. Smith, the able artist of Sweet's *Brit. Fl. Garden.* The characters above given are very strong, and sufficient, if constant; but may they not have arisen from some imperfect state of cultivation, or untimely transplantation of the root, or from being kept too long out of the ground? It seems to be a dwarf species close to *N. angustifolius* of *Bot. Mag. t.* 193.

Obs. ulteriores.—N. angustifolio affinis, sed minor, et differt scapo compresso ancipiti striato crassiore dodrantali, et forsàn pallidiore: *corollæ* laciniis latioribus minùs stellantibus; *coronâ* minore, erectâ, nec patulâ vel patellari, et potissimùm tubo laciniis duplò breviore, nec lacinias æquante; *ovario* ferè oblongo nec ovali, ut in *N. angustifolio.* Fortassè est *Narcissus odorus* circulo rubello, *Rudb. Elys. t.* 44. *cum icone.*

Genus PHILOGYNE.

Obs. Much yet remains to be cleared up in this genus.—*Philog. Campernelli* was described in *Narciss. Monog.* from imported roots, and had flowers lower than the leaves. Others exactly similar, but from Bury Botanic Garden, and kept in the ground all the year, had florigerous scapes higher than the leaves at the time of flowering; so the difference of relative altitude is thus accounted for: but the leaves in *P. Campernelli* were much paler, especially at the base, than those of *P. odorus*, and the flowers had little or no scent. I had only one specimen of *P. odorus* to compare with many of *P. Campernelli*, in the blooming season of the present year.

Ph. *Curtisii. Narciss. Monog. ed.* 2. *p.* 12.—This was described from a single specimen in Mr. Sweet's garden, and has not since bloomed; but he joined me in thinking it had more deep and distinct lobes on its ample and not rugulose, but crisped crown, than *Ph. interjectus;* and time may prove it.

From *Phil. triloba* it differs in the larger proportions of its crown.

POSTSCRIPT.

Genus Molium *mihi.* Allium *Linn., Treviran., Don, &c.*

Spatha 2—3-loba, marcescens. *Stamina* subulata, basi dilatata, plana, connata. *Scapi* nudi, centrales. *Folia* radicalia lorato-attenuata ecarinulata.

Obs. The genus *Moly* of Theophrastus, Dioscorides, &c., and all the higher divisions of my friend Mr. Geo. Don's very excellent Monograph on *Allium*, are good and sufficient *genera*, or *subgenera*, of the great family of *Alliæan* plants; and the very characters he has there laid down are sufficient and satisfactory. In the present place I have not leisure to dwell further upon this point, than merely to describe, as a distinct species of *Molium*, (sent to me as an *Ornithogalum*,) which Mr. Don, at the time of printing his elaborate Monograph, had no opportunity of separatng from *Molium nigrum* of Linnæus,—the *Allium multibulbosum* of *Jacq. Austr. t.* 10., and which by way of contrast may be called,

paucibulbosum. Molium (pale blush-coloured) inodorum: bulbo principì magno simplicitèr dichotomo sub-ebulbilloso, foliis erecto-incurvis glauculis, stylis 3—4, totidemque ovarii loculis.

Allium bicorne, latifolium, flore magno dilute purpurascente, *Rudb. Elys.* 11. *p.* 160. *f.* 21.

M. indicum flore purpureo, *Swert. Florileg. t.* 61, *figura centralis.*

M. montanum latifolium purpurascens Hispanicum, *Park. Parad. p.* 144?

Moly latifolium hispanicum, *Bauh. Pin. p.* 75. *n.* 111.

Obs. Planta inodora autumno quiescens ♃. *Radix* bulbosus magnus sine ferè bulbillis. *Folia* radicalia suberecto-incurva pedalia et ultrà, 2 uncias lata, lanceolato-lorata, glabra, incurvo-canaliculata, et indè basin versus præcipuè canaliculata, ad oras in lente

albo-marginulata et minutissimè asperiuscula. *Scapus* teres solidus plùs quàm 2-pedalis lævis aphyllus viridis. *Spatha* multiflora membranacea persistens et recurva lobulata. *Flores* capitatìm umbellati numerosissimi conferti albi, sed mox dilutè erubescentes. *Pedicelli* 1—1½-unciales teretes virides. *Perigonium* 6-petaloideum. *Petala* oblongo-lanceolata dorsali lineâ viridi, horizontalitèr expansa obtusa vel parùm incurvula. *Stamina* patula, filamenta subulata supernè plùs minùs erubescentia, *antheris* ordinariis. *Ovarium* lætè perviride obtusè 3—4—5-gonum, totidem loculis. *Styli* itidem 3—5 erecti subulati, sublineam longi albi, *stigmatibus* ferè nullis.

Proximum est *A. multibulbosum Jacq.*, sed differt in bulbo, foliis, floribusque; et potissimùm in *stylorum* numero, loculisque ovarii viridissimi.

In the course of composing my MS. *Hortus* (*sub dio*) *bulbosus*, I have found that *Zephyranthes grandiflora* of *Bot. Mag. tab.* 902. is the same plant as *Habranthus robustus* of Herbert, figured in *Sweet's Brit. Fl. Gard. ser.* 2. *tab.* 14. This may be useful for all botanists, bulb-growing collectors, and gardeners to know, as will also be the following remarks.

plumbea. Scilla. *Bot. Reg. tab.* 1355, is the same as *Scilla hyacinthoïdes Linn.* and *Bot. Mag. tab.* 1140.

esculenta. Scilla. *Missouri Squill* or *Quamash*, *Bot. Mag. tab.* 1574, must be very different, and not even of the same *genus*, as the *Camassia esculenta* (*Eatable Quamash*) of *Bot. Reg. tab.* 1486; and I think I have seen a smaller kind (perhaps only a weaker specimen,) of the former, which is a true species of *Scilla*.

Under the same useful views I may remark, that *Scilla unifolia Linn.—Lowd. Hort. Brit.* and *Scilla pumila Brot.* (*monophylla Link*) and *Sc. pumila Bot. Mag. t.* 3023. are the same plant.

I have long had in my garden two species of *Scilla*, that I am not aware of having been distinguished in print from each other; viz.

peruviana. Scilla. *Linn. Sp. Pl.* 442. *et ejus Herb.*: foliis lorato-lanceolatis, thyrso multifloro conferto, bracteis, pedunculo imo longè brevioribus.

Scilla stellaris bœticus major sive peruanus, *Park. Parad. tab.* 125. *f.* 7.

β. minor, floribus albis.

præbracteata. Scilla (long bracted): foliis lorato-lanceolatis, thyrso multifloro conferto, bracteis imis pedunculo valdè longioribus.

Scilla peruviana *Bot. Mag. tab.* 749.

Genus Muscari *Clus.*, *Tournef.* Hyacinthus *Linn. &c.*

Obs. Under the name of *Hyacinthus botryoides*, and its varieties, have been confounded two distinct species, which I separate and endeavour to establish as follows:

botryoides. Muscari (The grape-flowered): corollis racemosis confertis globosis uniformibus, pedicellis florigeris subnullis, longioribus, foliis loratis inflexo-canaliculatis erectis, bulbillis radicalibus numerosis.

Hyacinthus botryoides. *Linn. Sp. Pl.* 455.

Muscari botryoides. *Sw. Brit. Fl. Gard. tab.* 15

β. azureum (bright blue). *Sw. l. c. t.* 15. *f.* a.—Hy. muscari γ. cœruleum majus *Linn. Hort. Clif.* 126. *et Herb. Linn.*

γ. pallidum (pale blue). *Sw. l. c. f.* c.—Hy. Musc. α. exalbidum *Linn. Hort. Clif. l. c.*

δ. carneum (pale blush). Hy. Muscari β. carneum minus *Linn. Hort. Cl. l. c.* Hy. botryoides flore albo-rubente *Park. Par. p.* 115.

ε. album (white). *Sw. l. c. f.* b.—*Park. l. c. t.* 113 *f.* 6.

ζ. ramosum (the branched). *Park. Parad. t.* 113. *f.* †.

Obs. The last very remarkable variety, and the blush-coloured one, I have never seen; but they cannot depend better than on the fidelity of Linnæus and Parkinson.

peduncularis. M. (The sub-blotched sky-blue): corollis laxiùs racemosis paucioribus, globosis uniformibus imis pedicellis brevioribus, foliis patulis lineari-loratis inflexo-canaliculatis, bulbillis radicalibus numerosissimis.

Hyacinthus botryoides *Curt. Bot. Mag. t.* 157. *nec Linn. vel Sweet.*—Hy. botryoides cœruleus amœnus (The sky-coloured grape flower). *Park. Par.* 114. *t.* 113. *f.* 5. *cum optimâ descriptione et figurâ.*

Obs. Priori minùs et longè rariùs floret, nisi bulbilli innumeri removendi sunt, in quolibet anno.

Chelsea, July 1, 1832.

LII. *Notice of some recent Observations of* Encke's *Comet, and of* Gambart's *Comet of July* 19;—*extracted from a Letter from* Professor Schumacher *of Altona, to the Rev.* T. J. Hussey*.

ENCKE'S comet has been but twice observed this year (at least I have received no more observations), and that not in Europe, but by M. Mossotti at Buenos Ayres. It was uncommonly faint, and even there scarcely visible.

Of the comet discovered July 19 by M. Gambart, one of my assistants, Mr. Peters, has calculated the following elements, which he is now about to correct by the *whole* of the observations. They are founded upon Gambart's observations, July 19; our own here, August 4; and Nicolai's, August 21.

Passage . .	1832. Sept. 25·62887 mean Berlin time.	
Perihelion . .	227° 50′ 2″	counted from the apparent Equm July 29.
☊	72 28 16	
i	43 20 20	
log q	0·0728427	
	Retrograde.	

They represent the observations as follows:

	Diff. in Long.	in Latit.
July 19, Marseilles	−4″	− 1″
August 4, Altona	+1	+11
——21, Manheim	+2	+ 1

Altona, Sept. 3, 1832.

LIII. *On Periodical Variations in the Quantities of Water afforded by Springs;—in a Letter to* Sir Charles Lemon, *Bart. M.P. F.R.S. By* W. J. Henwood, *Deputy Assay-Master of the Duchy of Cornwall, F.G.S. Hon. M.Y.P.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE only publications pretending to bring this subject to numerical accuracy, with which I am acquainted, are Mr. Bland's† and my own‡. The information contained in both these, is of a very similar character; but I will now en-

* Communicated by Mr. Hussey.

† Phil. Mag. and Annals, N.S. vol. xi. p. 88: Read to the Geological Society, December 14th, 1831.

‡ Phil. Mag. and Annals, N.S. vol. ix. p. 170. Read to the Royal Society some time in February or March 1830. A typographical error, dates this in the Phil. Mag. January 28th, 1831, instead of January 28th, 1830, which it should have been.

deavour, as briefly as I can, to supply other matter, which I was prevented from giving in that paper, by circumstances not under my own control. The observations to which I now venture to solicit your attention, extend through seven years; from 1823 to 1829 both inclusive. The first inquiry seems to be, "For the same mine the depth being constant, what is the variation in the quantity of water delivered, for corresponding months of various years?" The examples I select, are the United Mines, situated in slate, depth from which the water is drawn 98 fathoms; Dolcoath, of which the surface is slate, and the bottom granite, water drawn from 222 fathoms deep; and Huel Damsel, situated in granite, depth 202 fathoms. For further details of the depth, and mode of estimating the water, I refer to my former paper*. The following Table I. represents the mean of the average monthly quantities of water pumped out of these mines, in cubic feet, and the monthly averages of rain in inches for the same periods.

TABLE I.

Mines.	January.	February.	March.	April.	May.	June.
	cubic feet.	cubic feet.	cubic feet.	cubic feet.	cubic feet.	cubic feet.
United Mines	4723799	5436565	5857656	4895049	5466032	4394400
Dolcoath	1866109	1723514	1782565	1722695	1741244	1645025
Huel Damsel	865274	749640	880608	807545	840111	839807
Rain	4·583	3·836	3·577	3·008	3·007	2·522
	July.	August.	Sept.	October.	Nov.	Dec.
United Mines	4425479	3887360	3436863	3435319	3510144	3330284
Dolcoath	1461295	1410007	1400135	1436790	1422857	1750345
Huel Damsel	731165	716970	683521	669309	653564	780149
Rain	2·882	3·985	3·934	4·928	4·194	5·543

Taking the foregoing means respectively as standards of comparison, we shall see how the monthly averages in the following Table II. vary therefrom.

* Phil. Mag. and Annals, N.S. vol. ix. p. 171.

	Surface above sea-level.	Bottom below sea.	Of the bottom full of water.
United Mines ...	48 fathoms.	160 fathoms.	120 fathoms.
Dolcoath	62	188	20 ......
Huel Damsel	59	143	

TABLE II.

	United Mines.§	Dolcoath.	Huel Damsel.	Rain.		United Mines.	Dolcoath.	Huel Damsel.	Rain.
1822.					1826.				
Dec.	...	1,894,762	765,677	2·545	June	2,205,913	1,526,805	1,223,887	0·66
1823.					July	3,892,417	1,271,191	662,709	1·24
Jan.	...	1,691,011	819,741	8·625	Aug.	3,419,060	1,152,142	626,789	2·115
Feb.	...	1,836,030	800,609	6·965	Sept.	2,872,736	1,237,470	588,744	4·315
Mar.	...	1,922,201	893,625	3·205	Oct.	2,759,297	1,567,249	431,676	2·84
April	...	1,567,387	793,119	1·98	Nov.	2,424,979	1,094,771	538,701	4·28
May	...	1,611,828	731,815	2·77	Dec.	2,462,892	1,470,081	590,235	4·4
June	...	1,608,971	675,959	1·94	1827.				
July	...	1,431,062	658,345	3·03	Jan.	3,316,010	1,714,405	683,331	4·42
Aug.	...	1,553,293	577,427	7·085	Feb.	3,636,077	1,690,749	603,554	1·6
Sept.	...	1,529,559	565,321	3·645	Mar.	4,548,457	*	740,237	5·265
Oct.	...	1,609,139	598,025	8·21	April	4,710,403	1,752,665	721,023	1·36
Nov.	...	1,710,348	644,470	3·705	May	4,350,315	2,091,307	859,778	5·315
Dec.	...	1,979,536	728,529	6·295	June	3,763,764	1,815,024	727,776	2·16
1824.					July	3,726,056	1,515,085	689,101	2·25
Jan.	...	1,808,211	785,731	2·005	Aug.	3,369,586	1,418,579	687,798	2·865
Feb.	...	1,570,986	709,011	3·28	Sept.	2,899,215	1,135,286	633,469	2·82
Mar.	...	1,714,520	729,649	5·74	Oct.	2,704,164	1,167,132	649,128	6·105
April	...	1,815,507	729,621	2·6	Nov.	2,552,262	1,245,571	644,479	3·165
May	...	1,576,255	698,081	3·955	Dec.	3,354,116	1,828,735	804,390	7·58
June	...	1,561,641	653,738	4·66	1828.				
July	...	1,635,458	714,421	3·18	Jan.	6,109,511	2,346,045	941,755	7·455
Aug.	...	1,551,167	705,689	2·6	Feb.	7,793,133	2,081,595	880,487	4·01
Sept.	...	1,367,813	652,500	4·165	Mar.	6,355,808	2,050,179	914,075	3·445
Oct.	...	1,401,820	639,811	5·965	April	5,193,102	*	871,083	7·5
Nov.	...	1,537,290	730,284	6·91	May	5,910,392	2,155,227	992,297	3·765
Dec.	...	1,872,764	857,628	6·47	June	5,874,431	2,180,986	962,159	1·955
1825.					July	5,306,100	1,501,722	933,176	4·55
Jan.	...	1,852,679	866,815	3·375	Aug.	4,481,789	1,466,902	1,004,299	4·575
Feb.	...	1,505,560	758,924	2·7	Sept.	3,722,706	*	974,995	3·045
Mar.	...	1,612,661	836,711	3·61	Oct.	3,672,149	1,560,404	958,115	3·255
April	3,755,247	1,705,725	735,568	1·005	Nov.	3,496,458	1,381,751	*	3·835
May	6,464,158	1,459,578	711,555	4·085	Dec.	4,173,845	1,609,129	1,029,026	6·225
June	5,800,654	1,406,288	687,904	1·8	1829.				
July	5,135,800	1,472,770	695,021	0·31	Jan.	4,745,878	1,843,533	1,154,831	3·13
Aug.	4,433,788	1,386,542	642,376	3·14	Feb.	*	1,523,075	*	3·005
Sept.	3,877,763	1,244,851	585,315	2·73	Mar.	5,071,161	1,910,332	1,164,264	1·465
Oct.	3,745,016	1,244,948	577,089	5·09	April	4,579,744	*	1,014,850	5·5
Nov.	4,732,704	1,284,167	555,002	5·82	May	5,328,338	1,572,507	1,110,616	0·56
Dec.	*	1,597,410	685,556	5·285	June	4,327,238	1,415,462	946,224	4·48
1826.					July	4,067,023	1,461,774	765,383	5·615
Jan.	*	1,806,884	804,716	3·075	Aug.	3,732,569	1,341,428	774,412	5·515
Feb.	4,917,809	1,856,601	741,670	5·295	Sept.	3,811,897	1,885,834	784,308	6·82
Mar.	7,417,875	1,485,500	889,283	2·31	Oct.	4,295,971	1,506,842	831,217	3·035
April	6,236,749	1,772,190	787,555	1·11	Nov.	4,344,319	1,706,102	808,451	1·645
May	5,276,957	1,722,006	776,638	0·6					

§ My observations on this mine are not comparable with one another further back than April 1825.

* No observations recorded for the months thus distinguished.

Collecting the annual results of this Table, it is found that the cubic feet of water drawn, and rain in inches, are,

TABLE III.

Year.	United Mines.	Dolcoath.	Huel Damsel.	Rain.
1823	*	19,965,691	8,524,133	53·705
1824	*	19,520,204	8,477,065	51·355
1825	*	18,048,533	8,510,008	40·135
1826	49,477,875	18,090,219	8,757,924	33·125
1827	42,039,201	18,798,449	8,229,909	41·725
1828	61,269,695	21,676,376	10,890,395	54·97
1829	53,914,548	19,558,583	11,123,222	46·995

From Table I. it is obvious that for the seven years under consideration, the maximum of rain has been in December, and the minimum in June†.

Mines.	Greatest Quantity of Water.	Months after Max. Rain.	Smallest Quantity of Water.	Months after Min. Rain.	Max. of Water drawn, exceeds Min. of the mean by.
United Mines	March	3	Dec.	6	0·574
Dolcoath	Jan.	1	Sept.	3	0·288
Huel Damsel	March	3	Nov.	5	0·295

I do not think any of the foregoing Tables afford information on which we can build, of the effects of rain on springs rising at great depths; for in Table III. an inverse ratio between them seems to obtain about as frequently as a direct one. Nor does this appear to proceed from any extraordinary drought or flood towards the termination of a preceding year.

The second point to be investigated is, *In a given mine, what periodical variation in the quantity of water obtains for any determinate increase of depth?* Seeing the difficulties with

* For these unreported months, I have, to complete Table III., interpolated from Table I.

† Mr. Giddy, from whose register the rain is taken, gives the minimum rain for 1821, 1822, 1823, 1824, 1825, 1826, 1827 in April, Phil. Mag. and Annals, N.S. vol. iii. p. 181.

The difference between the mean temperature of the years of these observations are:

1823	mean	51°·	1827	mean	51°·5
1824	—	51·5	1828	—	52·2
1825	—	52·	1829	—	50·
1826	—	53·5			

Giddy, Phil. Mag. and Annals, N.S. vol. iii. p. 182.

which we had to contend, in the first part of this inquiry, it is obvious that the present one presents many others, some of which are to me insurmountable. I take as examples situated in *clay slate*, Huel Rose, surface 35? fathoms above the sea; Huel Hope, surface 50? fathoms above that level; and Poldice, surface from 28 to 40 fathoms above the sea. In *granite* the only case I shall adduce is Huel Reeth, of which the surface is about 66 fathoms above sea mark†.

TABLE IV. ‡

	Huel Rose.		Huel Hope.§			Huel Rose.		Huel Hope.	
	Depth Fath.	Water, Cub. Ft.	Depth Fath.	Water, Cub. Ft.		Depth Fath.	Water, Cub. Ft.	Depth Fath.	Water, Cub. Ft.
1824.					1827.				
Aug.	55	1,565,187	...	...	April	84	2,433,535	77	3,073,158
Sept.	...	1,545,203	...	...	May	91	2,387,409	...	2,873,097
Oct.	59	1,626,549	...	...	June	94	2,253,363	...	2,899,681
Nov.	63	1,801,256	...	...	July	...	2,318,711	...	2,833,634
Dec.	64	2,696,699	...	...	Aug.	...	*	...	2,553,753
1825.					Sept.	...	1,910,871	...	2,273,581
Jan.	...	2,646,157	...	...	Oct.	...	1,854,494	...	2,225,924
Feb.	...	2,341,190	...	...	Nov.	...	*	...	2,309,194
Mar.	...	2,333,533	...	...	Dec.	...	*	88	2,984,186
April	70	2,250,359	...	...	1828.				
May	...	*	...	...	Jan.	85	3,174,525	...	*
June	...	1,879,489	48	1,679,843	Feb.	90	3,236,128	108	3,292,749
July	72	1,735,859	53	1,482,723	Mar.	...	3,276,967	...	3,125,796
Aug.	...	1,610,751	55	1,459,652	April	100	3,001,722	...	2,872,231
Sept.	75	1,555,045	56	1,335,605	May	...	3,072,177	...	2,874,119
Oct.	...	1,615,076	...	1,369,297	June	95	2,807,398	112	2,629,117
Nov.	...	1,637,411	...	1,346,398	July	100	2,673,048	...	2,662,792
Dec.	...	1,851,747	66	2,066,255	Aug.	95	2,530,456	112	2,501,831
1826.					Sept.	...	2,430,522	...	2,264,919
Jan.	...	2,296,465	74	2,428,149	Oct.	...	*	...	2,281,205
Feb.	...	2,039,894	71	2,123,209	Nov.	...	*	...	2,146,405
Mar.	...	2,459,984	...	2,562,010	Dec.	...	*	...	2,388,438
April	...	1,913,890	...	2,309,617	1829.				
May	...	*	77	2,092,514	Jan.	90	2,542,354	...	2,889,535
June	...	1,859,413	...	*	Feb.	...	2,545,047	...	2,748,953
July	...	1,851,513	...	1,766,160	Mar.	...	2,863,117	...	2,788,771
Aug.	...	1,751,822	...	1,677,752	April	104	2,692,850	112	2,659,448
Sept.	...	1,682,792	...	1,584,818	May	107	2,728,049	...	2,788,974
Oct.	85	1,835,665	...	1,669,365	June	111	2,585,863	...	2,716,185
Nov.	...	*	...	*	July	113	2,525,173	128	2,285,164
Dec.	...	*	...	2,414,911	Aug.	122	2,453,833	...	2,231,667
1827.									
Jan.	79	2,168,204	...	2,699,121	Sept.	125	2,357,154	...	2,242,170
Feb.	84	1,711,032	...	2,536,137	Oct.	...	2,628,751	...	2,621,985
Mar.	...	*	...	3,362,051	Nov.	...	2,740,998	...	2,489,828

† Phil. Mag. and Annals, N.S. vol. ix. p. 171.

‡ I make two Tables, IV. and IV a. because it appeared that the first two and the second two formed series very distinct, so far as *depth*, at least. The rain having been given in Tables I. and II., I do not repeat it.

§ Phil. Mag. and Annals, N.S. vol. ix. p. 172. The columns mean, depth of the mine, in fathoms; and quantity of water drawn out, in cubic feet.

TABLE IV².

	Poldice. Depth Fath.	Poldice. Water. Cub. Ft.	Huel Reeth. Depth Fath.	Huel Reeth. Water. Cub. Ft.
1822.				
Dec.	135	*	125	880,663
1823.				
Jan.	...	3,327,107	...	629,219
Feb.	...	4,924,641	...	915,563
Mar.	139	3,940,670	...	738,615
April	...	3,303,934	...	429,341
May	141	3,033,058	127	342,207
June	142	2,716,302	135	273,723
July	144	2,521,868	...	216,365
Aug.	145	2,501,825	...	302,189
Sept.	146	2,437,174	...	379,582
Oct.	147	2,530,667	145	523,531
Nov.	...	2,679,540	...	575,191
Dec.	...	2,961,414	149	661,269
1824.				
Jan.	...	3,244,887	...	584,321
Feb.	...	2,943,396	...	405,159
Mar.	...	3,350,807	...	571,395
April	...	3,151,160	...	376,196
May	...	3,162,396	...	374,940
June	...	2,862,492	...	342,268
July	...	3,095,589	...	420,928
Aug.	...	2,737,222	...	303,117
Sept.	...	2,594,766	...	263,169
Oct.	...	2,836,271	...	401,328
Nov.	...	2,781,280	...	536,512
Dec.	...	3,499,641	...	842,607
1825.				
Jan.	142	3,640,529	...	633,158
Feb.	...	3,274,633	...	610,574
Mar.	...	3,481,963	...	557,904
April	151	3,243,756	...	345,880
May	152	3,383,893	...	339,126
June	...	3,256,043	...	288,918
July	...	2,728,108	...	259,408
Aug.	...	2,318,724	...	213,110
Sept.	...	2,242,829	...	193,304
Oct.	...	2,438,833	...	208,976
Nov.	...	2,262,912	...	326,315
Dec.	157	2,700,186	169	563,006
1826.				
Jan.	...	2,763,869	...	503,617
Feb.	...	2,612,800	...	605,474
Mar.	...	3,560,152	...	579,844
April	...	2,504,400	...	360,055
May	...	2,781,242	...	282,815

	Poldice. Depth Fath.	Poldice. Water. Cub. Ft.	Huel Reeth. Depth Fath.	Huel Reeth. Water. Cub. Ft.
1826.				
June	157	2,660,942	169	304,203
July	162	2,682,468	...	262,885
Aug.	.	2,342,342	...	225,141
Sept.	...	2,025,384	...	209,518
Oct.	...	2,153,458	...	206,009
Nov.	...	2,123,368	...	198,651
Dec.	...	2,479,338	..	478,529
1827.				
Jan.	...	2,319,208	...	489,605
Feb.	...	2,579,821	...	467,429
Mar.	...	3,187,412	...	721,001
April	...	2,961,881	...	433,656
May	..	3,185,067	...	524,469
June	...	2,921,629	...	420,608
July	...	2,717,207	...	334,130
Aug.	...	2,723,087	...	241,958
Sept.	...	2,255,414	...	257,111
Oct.	...	2,591,115	...	332,998
Nov.	...	2,668,019	...	498,538
Dec.	...	2,887,534	...	824,990
1828.				
Jan.	...	4,192,716	...	999,368
Feb.	...	3,597,390	...	*
Mar.	...	3,499,163	...	488,384
April	...	3,206,197	...	533,962
May	...	3,330,326	..	459,621
June	...	*	...	406,112
July	...	3,303,130	...	339,984
Aug.	...	3,160,195	...	378,825
Sept.	...	2,814,212	...	323,576
Oct.	...	2,756,998	...	363,892
Nov.	166	2,308,730	...	428,613
Dec.	167	2,871,306	...	598,067
1829.				
Jan.	...	*	...	484,343
Feb.	...	2,801,183	...	426,356
Mar.	...	*	...	435,451
April	171	2,921,596	189	433,665
May	...	3,222,374	...	413,624
June	...	2,875,565	...	333,116
July	...	2,921,754	...	373,325
Aug.	172	2,651,104	...	443,614
Sept.	...	2,801,815	...	629,729
Oct.	...	3,258,694	...	557,110
Nov.	...	3,199,579	...	442,601

The results of Tables IV. and IV². afford the basis for the following *monthly* mean for the various mines.

* No observations recorded for the months thus distinguished.

TABLE V.

Mines.	Mean Depth. Fath^{ms}	January.	February.	March.	April.	May.	June.
Huel Rose	86	2,934,372	2,555,207	2,979,034	2,773,150	2,922,359	2,389,601
Huel Hope	90	2,672,268	2,658,524	2,976,395	2,728,614	2,657,176	2,481,206
Poldice.....	156	3,248,052	3,233,662	3,519,732	3,041,846	3,156,908	2,882,162
Huel Reeth	160	617,661	559,848	600,701	416,108	390,972	338,421
		July.	August.	Sept.	Oct.	Nov.	Dec.
Huel Rose	86	2,220,861	1,982,410	2,012,454	2,056,895	2,183,835	2,954,850
Huel Hope	90	2,206,094	2,084,932	1,940,218	2,033,555	2,072,956	2,463,448
Poldice	156	2,852,875	2,633,499	2,453,085	2,652,291	2,574,775	2,899,903
Huel Reeth	160	315,289	301,136	322,284	370,549	429,489	692,733

From Tables IV. and IV². we also obtain the *annual* results contained in

TABLE VI.†

Year.	Huel Rose.		Huel Hope.		Poldice.		Huel Reeth.		Rain.
	Mean Depth.	Water.	Mean Depth.	Water.	Mean Depth	Water.	Mean Depth.	Water.	
1823	*	*	*	*	141	37,361,088	132	6,206,189	53·705
1824	*	*	*	*	147	35,721,680	149	5,240,602	51·355
1825	70	24,717,645	*	*	149	35,772,864	149	4,819,280	40·135
1826	77	23,561,261	74	24,050,960	159	30,910,611	169	4,301,218	33·125
1827	90	25,717,144	77	32,054,242	162	32,589,198	169	5,200,032	41·725
1828	95	33,487,188	107	32,645,018	162	38,185,210	169	6,193,176	54·97
1829	108	31,808,118	119	30,851,118	170	36,566,916	182	5,571,001	46·995

These results are obviously dependent on two causes of variation;—first, the variable quantity of rain; and, second, the difference in depth at various times.

Having already discussed the former, we are enabled to use the data obtained in Table III., having nothing better to apply. Huel Rose and Huel Hope are compared, so far as possible, with United Mines, being in the same stratum; and where the observations on the latter do not extend, I make the comparison between the former and Dolcoath. Poldice is also brought to the standard of Dolcoath. Huel Reeth (in granite) is reduced to the ratio of Huel Damsel.

† To complete this, I have been compelled to interpolate from Tables IV. IV². and V.

TABLE VII.*

Year.	Depth.	Calculated Quantity of Water, assuming Depth constant.	Difference† due to Increase in Depth.	Water given Table VI.
		Huel Rose.		
1823	...	......	......	......
1824	...	......	......	......
1825	70	24,717,645	......	24,717,645
1826	77	24,774,734	−1,213,473	23,561,261
1827	90	20,018,979	+5,698,165	25,717,144
1828	95	33,462,300	+ 24,888	33,487,188
1829	108	25,673,994	+6,134,124	31,808,118
		103,930,007	+10,643,704	114,573,711
		Huel Hope.		
1823	...	......	......	......
1824	...	......	......	......
1825	...	......	......	......
1826	74	24,050,960	......	24,050,960
1827	77	21,001,506	+11,052,736	32,054,242
1828	107	30,608,477	+ 2,036,541	32,645,018
1829	119	26,934,069	+ 3,917,049	30,851,118
		78,544,052	+17,006,326	95,550,378
		Poldice.		
1823	141	37,361,088	......	37,361,088
1824	147	36,527,464	− 805,784	35,721,680
1825	149	33,773,533	+1,999,331	35,772,864
1826	159	33,851,539	−2,940,928	30,910,611
1827	162	35,176,823	−2,587,625	32,589,198
1828	162	40,562,178	−2,376,968	38,185,210
1829	170	36,599,233	− 32,317	36,566,916
		216,490,770	−6,744,291	209,746,479
		Huel Reeth.		
1823	132	6,206,189	......	6,206,189
1824	149	6,171,920	− ,931,318	5,240,602
1825	149	6,195,905	−1,376,625	4,819,280
1826	169	6,376,406	−2,075,188	4,301,218
1827	169	5,991,972	− 791,940	5,200,032
1828	169	7,928,999	−1,735,823	6,193,176
1829	182	8,098,514	−2,527,513	5,571,001
		40,763,716	−9,438,407	31,325,309

* For want of better information, I have *assumed* the effects of rain at *all* depths to be *uniform*;—an assumption which I make without any intention to advance such an opinion, and which we have at present no sufficient knowledge either to confirm or disprove. It is also assumed that in all the mines in this Table (VII.), the *whole* of the first year's results depend on rain, and no part thereof on rain.—This also is merely taken for granted, in order to compare the other values with one another.

† When the observed quantities *exceed* the calculated, I prefix the +

The before-mentioned mines are selected *merely* because they afforded examples of the various results in the different strata of our mining districts. They appear to show,

For Mines of which the Depths are constant:

1. That although the rain falling appears to exert, after a certain time, some influence on the quantity of water drawn out of mines, yet the amount of this effect is not in a *direct simple* proportion.

2. That although *great* differences in the quantity of rain appear to modify the quantity of water in mines, yet the variations so induced, sometimes disappear when the differences of rain falling are *small*.

3. The times elapsing between the maxima and minima of rain, and those between the maxima and minima of water in the mines, are often not identical; nor are they always the same for *different mines*.

For Mines increasing in Depth:

4. From Tables IV. IV^2. V. VI. and VII. it would appear, that in mines from 70 to 120 fathoms deep, the quantity of water is increased as the depth is augmented; but that in others of from 130 to 180 fathoms in depth, an *augmented* depth induces a *diminution* in the quantity of water.

It is not my intention to insist on the universality of these inferences, which further investigations may, perhaps, show to be of but limited application. I regret that more pressing engagements at present forbid my entering further on the subject, to which however I hope on another occasion to return, with other matter, which is now in a state of preparation.

I have the honour to be,

Your most grateful humble Servant,

W. J. Henwood.

Perran Wharf near Truro, June 5, 1832.

LIV. *Chemical Researches on the Blood of Cholera Patients.* *By* Thomas Andrews, *Esq**.

THE discordancies in the various analyses which have been published of the cholera blood, rendering it desirable that the subject should be again investigated with precision, I availed myself of the opportunity afforded by the prevalence of the epidemic in Belfast, to institute a new set of experiments.

The first analysis of cholera blood that appeared in this

sign to the numbers in this column; when the excess is on the other side, the — sign.

* Communicated by the Author.

country is Dr. Clanny's; from which, and a corresponding analysis of the blood in health, he inferred, that the water, as well as the albumen and fibrin, are deficient in quantity; that the colouring matter, and what he denominates free carbon, are greatly in excess, and that the saline constituents are entirely wanting. Dr. O'Shaughnessy is the next chemist who turned his attention to the subject; but he seems to have confined himself principally to the serum, of which he has published a very elaborate examination. He found its specific gravity increased in consequence of the deficiency of water, the animal matter considerably in excess, but a diminution of the salts, especially of the carbonate of soda, which in one case was absent, the serum being devoid of action on test paper. But the latest, and by far the most valuable researches on cholera blood are those of Dr. Thomson, which, although they do not exhibit its true composition, yet furnish data from which it may be nearly calculated. He agrees with Dr. Clanny in the excess of colouring matter and deficiency of water, albumen and fibrin (but he confesses that the deficiency of the latter may be doubted) in the blood; while in the serum he found the albumen increased, but the salts normal in amount and composition*.

A few of these different results may have arisen from variations in the composition of the specimens of blood which were subjected to analysis; others can be referred only to errors of experiment; but the principal source of them is the diversity of the modes of analysis which were followed. It is for this latter reason that I shall enter with more minuteness than might otherwise be necessary into the details of the following experiments.

Specimen 1. Cholera Hospital, Belfast.—This was obtained from a rapid case of cholera; but I know nothing more of its history.

The blood was taken from the vena cava immediately after death, and introduced into a vial, in which it afterwards coagulated. The serum was slightly tinged red, but perfectly transparent; the crassamentum was not in this case darker than it often appears in healthy blood. Their relative proportions were

Serum	41·6
Crassamentum	58·4
	100·0

* An abstract of Dr. O'Shaughnessy's results will be found in Phil. Mag. and Annals, N.S. vol. xi. p. 469: Dr. Thomson's researches on cholera blood were published in the same volume, p. 347. Edit.

But as the serum was merely drawn off, these proportions do not admit of comparison with the healthy ratio of Berzelius.

Serum.—Specific gravity 1·038; had an alkaline reaction. 32·518 grammes of it were evaporated to dryness; and after being reduced to a coarse powder, dried till they ceased to lose weight on a warm bath, the temperature being prevented from rising too high by placing some shreds of cotton beneath the capsule containing the albumen. The dried mass weighed 4·078 grammes. This was now incinerated and washed repeatedly with boiling water, which was evaporated to dryness, and the saline matter thus obtained calcined, and found to weigh ·243 gramme, to which, adding ·027 (obtained, as we shall hereafter mention), we have the saline matter soluble in water equal to ·27 gramme.

About ·02 gr. of this matter was carefully examined to determine its nature. By spontaneous evaporation it yielded a set of crystals, which, examined by a microscope, proved to be principally cubes intersected by others of an acicular form. Two or three of the largest and purest of these cubes were dissolved in water; the solution had a strongly alkaline reaction, and was precipitated by nitrate of silver, the precipitate being soluble in ammonia. The rest of the crystals were now dissolved in a drop of water: pure ammonia was added to a minute portion of it, and a faint cloud appeared, indicating the presence of phosphoric acid. The remainder of the solution was divided into two portions; to one of which tartaric acid was added, and to the other chloride of platinum. Evolution of gas took place in both cases; and in the one solution, numerous clusters of crystals (whose shape was a six-sided prism) appeared in a few seconds; while in the other a granular deposit of octohedral crystals was soon formed.

To the remaining ·25 gr. of saline matter, chloride of barium was added in excess: a white precipitate fell, but the solution continued alkaline, and by evaporating it to dryness, a portion of insoluble matter remained, which had principally arisen during the evaporation, forming a thick crust on the surface of the liquid. The solution still continued slightly alkaline, and became opake on the surface. These experiments indicate the presence of uncombined alkali. The carbonate of baryta weighed and estimated for the whole saline matter was equal to ·0972 gramme, equivalent to ·0525 carbonate of soda. It dissolved with effervescence in nitric acid, leaving a residue of ·012 of sulphate of baryta, equivalent to ·008 of sulphate of potash. The nitric acid solution was precipitated by ammonia, and prussiate of potash occasioned a faint white cloud.

In order to obtain the remainder of the saline matter from the incinerated mass, it was boiled in acidulated water, ·050 gr. of saline matter was obtained, of which ·027 gr. dissolved in water. The remainder was dissolved in nitric acid and precipitated by ammonia of a slightly red colour, then redissolved in nitric acid and precipitated by oxalate of ammonia; but I could not detect any magnesia in it, probably from the minute scale on which the experiment was performed.

The serum therefore contained,

Water	874·59
Albumen	116·40
Chlorides of sodium and potassium with uncombined alkali	6·69
Carbonate and phosphate of soda	1·36
Sulphate of potash	·25
Phosphate of lime	·71
	1000·00

Crassamentum.—23·49 grammes were dried in the same manner as the albumen; they lost 16·472 gr. of water. This water arose from the serum in the crassamentum, and must have been united, by its analysis, to 2·360 gr. of albumen and salts. Hence the crassamentum consisted of 18·832 serum, and 4·658 red globules and fibrin.

58·58 grammes of the same crassamentum were washed to separate the fibrin, but the process was very tedious; and after persevering for above a week, I did not succeed in rendering the fibrin perfectly colourless. It was dried at the same temperature as the albumen and crassamentum. It weighed ·52 gr. and was of a dirty green colour.

From these experiments the composition of the blood was,

Water	78·43
Albumen and salts	10·00
Red globules	11·06
Fibrin	·51
	100·00

Specimen 2. Cholera Hospital, Ballymacarrett.—This specimen of blood was taken from a male patient (æt. 50), who had been seized with cholera the same morning, and died early on the following day. From the commencement of the attack he had passed involuntary stools, and vomited copiously. The pulse was perceptible before he was bled, but afterwards became very faint and irregular. The blood flowed with difficulty, and was of a very dark colour and viscid consistence. It coagulated perfectly, the serum was

yellow and pure, and the crassamentum much darker and more bulky than usual*.

Serum.—Sp. gravity 1·045; alkaline reaction; taste saline, similar to healthy serum. 14·377 grammes of it were analysed in precisely the same manner as the preceding specimen. Its constituents were,

Water	847·02
Albumen	144·36
Chlorides of sodium and potassium, with free alkali	5·96
Carbonate and phosphate of soda	1·62
Sulphate of potash	·22
Phosphate of lime with a trace of iron	·82
	1000·00

Crassamentum.—The blood weighed 77·94 grammes, and the crassamentum, with a considerable portion of impure serum, 58·27 gr. The latter contained 47·604 gr. of serum, and ·231 gr. of fibrin, of a buff colour and pretty pure. The composition of the blood was therefore,

Water	73·11
Albumen and salts	13·21
Red globules	13·38
Fibrin	·30
	100·00

Specimen 3. Lunatic Asylum, near Belfast.—This was taken from a female patient (æt. 20) in the state of collapse, the radial pulse not being perceptible when the blood was drawn. It flowed in a continuous stream for a few seconds, but afterwards trickled with extreme difficulty. The patient died next day. The blood was black and thick; it coagulated as usual.

Serum.—Sp. gravity 1·040; of a pure yellow colour; 4·811 gr. left, by desiccation, ·636 gr. of albumen and salts. It contained therefore,

Water	865·95
Albumen and salts	134·05
	1000·00

The saline matter was not weighed, but its solution was alkaline, and effervesced with acids.

* In this, as well as in all the following cases, the blood was received into a vial, which was immediately closed. This precaution was necessary, as serum exposed to the air evaporates with great rapidity.

Crassamentum.—The proportion of serum to crassamentum was,

Serum	40·5
Crassamentum	59·5
	100·0

But the same observation applies to this as to the former determination.

The crassamentum contained 68·55 per cent. of water; it contained also ·075 gr. of pure fibrin, equivalent to ·26 per cent., the blood weighing 28·937 gr. Hence it consisted of,

Water	74·93
Albumen and salts . . .	11·60
Red globules	13·21
Fibrin	·26
	100·00

Specimen 4. Lunatic Asylum.—This blood was drawn from the jugular vein of a female patient (æt. 20), who had rallied from collapse for about a day by artificial excitement, the blueness having disappeared, and the natural warmth having been restored. The blood was obtained six hours after death. It did not coagulate, but the red globules subsided, leaving the serum yellow and pure.

Serum.—Sp. gravity 1·040. 9·940 grammes of it were subjected to analysis, and found to contain,

Water	866·72
Albumen and salts . . .	133·28
	1000·00

The saline matter was found to be about 1·2 per cent., but the experiment was not made with much precision: its solution in water was alkaline, effervesced with acids, and contained both potash and soda.

The blood was found to contain,

Water	76·07
Albumen and salts . . .	11·69
Red globules	12·24
	100·00

There was no fibrin present.

Having thus ascertained the composition of the blood in the severer stages of the complaint, I next proceeded to examine it in the incipient stages.

Two of the first specimens I procured were from the Cholera Hospital, taken from patients affected with diarrhœa and

vomiting, but who afterwards recovered. I did not see them myself, and therefore cannot be certain whether they were real cases of cholera or not: the specimens resembled in every respect healthy blood. The sp. gravity of the serum of one was 1·0243, and of that of the other 1·0232. The latter was subjected to analysis; it contained,

Water	919·99
Albumen	71·62
Salts	8·39
	1000·00

The serum was to the crassamentum in the ratio of 51·3 and 48·7, and the latter contained 74 per cent. of serum. Hence the blood was composed of,

Water	80·35
Albumen and salts . . .	6·99
Red globules	12·66
	100·00

Specimen 6. Ballymacarrett Hospital.—This was taken from a female (æt. 45), who had been affected with violent purging and vomiting. The pulse was feeble when the blood was drawn, but she did not fall into collapse. The blood coagulated as usual.

Serum.—Sp. gravity 1·031; very pure. It consisted of,

Water	891·69
Albumen and salts . . .	108·31
	1000·00

Crassamentum.—The fibrin in this case was determined by agitating a separate portion of the blood with a network of iron wire, and was thus readily obtained pure, and found to be ·296 per cent. The blood contained,

Water	77·93
Albumen	9·43
Red globules	12·34
Fibrin	·30
	100·00

In three other cases of incipient cholera the serum was found to have the following specific gravities.

Sp. gravity .	1·027
	1·030
	1·033

The last was from a very well marked case. These experiments on the blood of incipient cases, though less numerous

than I should have wished, seem to me to warrant the general conclusion, that the composition of the blood does not differ from the normal state during the early stages of the disease.

In order to show more clearly the changes induced in the blood by cholera, I shall collate the results of my own experiments with those obtained in the analysis of healthy blood.

SERUM.

	Health.	Cholera.	
	Sp. gravity 1·029.	Sp. gravity 1·038.	Sp. gravity 1·045.
Water	900·00	874·59	847·02
Albumen	90·80	116·40	144·36
Chlorides of sodium & potassium	6·60	6·69	5·96
Carbonate & phosphate of soda .	1·65	1·36	1·62
Sulphate of potash	·35	·25	·22
Earthy phosphates	·60	·71	·82
	1000·00	1000·00	1000·00

The analysis of healthy blood is Dr. Marcet's, which closely agrees with those of Berzelius and Lecanu. A glance at this table is sufficient to show that in the serum of cholera blood, the albumen is in great excess, but that the *salts are both qualitatively and quantitatively the same*, the minute differences in their proportions being less than analysts have found in healthy blood*.

BLOOD.

	Cholera.				Incipient Cholera.		Health. Prevost and Dumas.
	1.	2.	3.	4.	5.	6.	
Water	78·43	73·11	74·93	76·07	80·35	77·93	78·39
Albumen and salts	10·00	13·21	11·60	11·69	6·99	9·43	8·69
Red globules . . .	11·06	13·38	13·21	12·24	12·66	12·34	12·92
Fibrin	·51	·30	·26	...	...	·30	

* It may not be uninteresting to observe here, the striking analogy between these conclusions and those of Dr. Marcet; who found in the analysis of dropsical fluids, that however great the variation of albumen, the proportion of salts was invariably the same as in the serum of blood.

I shall venture to give one other table, because I believe its results have not been published in any English work, and they are essential to a correct knowledge of the composition of the blood.

	Male.		Female.	
	Max.	Min.	Max.	Min.
Water	85·31	79·04	80·53	77·86
Albumen and salts	8·74	6·72	9·23	6·68
Red globules and fibrin . .	13·00	6·83	14·84	11·58

The variation in cholera blood from the healthy standard is not so great as is generally supposed. The water is not only in every case below the mean of healthy blood, but below the minimum in the experiments of Lecanu, and the albumen is proportionally increased. In the analyses of Prevost and Dumas and of Lecanu, the fibrin was not separated from the red globules, nor do I know of any experiment worthy of confidence on the amount of the former constituent in healthy blood; it is generally estimated at about five per cent., but I am inclined to believe that it is not one tenth of that quantity. Indeed it is with diffidence that I publish the results I have obtained from cholera blood, as I am satisfied that the process suggested by Dr. O'Shaughnessy is the only one susceptible of precision. It was, however, followed in the last experiment, and the results agreed with those obtained by the other method. Another source of fallacy is this; —that the temperature at which fibrin is decomposed seems much lower than that necessary to decompose other animal principles: but further experiments are necessary to elucidate this point. I shall only further observe, that in the heart from which the specimen No. 1. was taken, scarcely a vestige of fibrin could be discovered.

But the most interesting and important fact derived from these investigations is, that, contrary to the conclusions of former experimenters, and apparently in direct opposition to the evidence of the senses, the colouring particles in black cholera blood exist in the same proportion as in the blood of health, varying not more than a half per cent. from the normal standard; and as a much greater diversity is found in the blood of different individuals in health, we must conclude that these slight variations are independent of, and unconnected with, its diseased state.

These results differ very much from preceding analyses, but it will not be difficult to reconcile them with the experi-

ments of Dr. Thomson. In order to separate the globules, he merely washed the crassamentum (drained of its serum), and evaporated the solution thus obtained; but it is evident that in this way "a portion of serum containing albumen," which could never be appreciated, varying with the bulk of the coagulum, "would be added to the colouring matter, and have the effect of apparently increasing its quantity." In one experiment, he found in this way the red globules to be 27·4 per cent.; while the albumen and salts only amounted to 5·9 per cent.: in another the red globules were 23·2 per cent., and the albumen 7·5. Fortunately, however, he has stated the water in the crassamentum, as well as its proportion to the serum, from which, and the composition of the serum, the relative proportion of the constituents of the specimens of blood which he analysed may readily be calculated as follows:

Water	70·76	67·96
Albumen and salts	13·53	15·83
Red globules	15·33	14·87
Fibrin	·38	1·34
	100·00	100·00

These results do not perfectly agree with my own experiments, but the coincidence is sufficient to confirm the deductions which I have made from them. The analysis of the serum by Dr. Thomson proves also that the salts are in every respect normal; and I cannot therefore avoid concluding that the experiments of Dr. O'Shaughnessy are inexact. Unless Dr. Clanny will publish with more detail the methods he has followed in analysing both healthy and diseased blood, it will be difficult to understand how he has arrived at his conclusions. It may be right, however, to observe, that the amount of residual carbon obtained by calcining albumen, globules, or any proximate principle, will not depend on the organic matter itself, but on the salts, and especially on the phosphates which may be present; for these by fusing protect the carbon from combustion; but if they are previously removed, then the "free carbon" of Dr. Clanny will speedily disappear by calcination even in a covered crucible.

If these experiments and those of Dr. Thomson can be relied upon, the principles upon which the saline treatment is founded are evidently false. To introduce a small quantity of inert saline matter into the stomach will certainly be as inefficacious in the cure of diseases, as it is innocuous: but it is a question of very great importance to determine, whether the addition of a large portion of salts to the blood by infusion into the veins (introduced with an intention of supplying a

deficiency, but in reality occasioning an excess,) may not only be not beneficial, but positively injurious. It is not improbable, that since so great a uniformity exists in the amount of saline ingredients in every variety of serous fluid, this quantity in the serum of blood may be essential to the due discharge of the functions of that fluid. An accurate examination of the blood in sea-scurvy might throw light on this obscure subject; for either the exhibition of saline remedies is an absurdity, or the serum of a scurvy patient is overcharged with salts. In dropsy the blood is drained of a fluid containing a much larger quantity of salts than the cholera evacuations (if the experiments of Dr. O'Shaughnessy on the latter be exact);—yet who will pretend to discover in such patients or in their blood any of those marvellous effects which have been attributed to the absence of these matters? The evacuations in cholera, containing little more than half the saline matter of the serum, ought to increase instead of diminishing its saline contents: but I do not doubt that if these evacuations could be obtained in the same state in which they are separated from the serum, and unmixed with other fluids, they would contain nearly the same proportion of salts which is found in it. There is one circumstance indeed which renders it improbable that even if a deficiency of salts could occur, it would produce any very injurious effect: the serum of a bullock, resembling in every other respect that of man, contains (according to Berzelius) less than half its saline ingredients; yet it is neither darker nor more difficult of arterialization. But we must not hence draw a hasty conclusion, that either a deficiency or excess of salts in the blood would be harmless.

The following are the general conclusions that appear to follow from these researches.

That the only difference between the blood of cholera and of health consists in a deficiency of water in the serum, and a consequent excess of albumen.

That the saline ingredients of the serum are the same as in healthy blood.

That the red globules, and probably the fibrin also, are normal.

That the want of fluidity in the blood, the darkness of its colour, and the bulk of the crassamentum, are simple effects of the increased viscidity of the serum.

I am at present engaged in further researches connected with this subject; but conceiving these results to be of some immediate interest, I have been induced to publish them in this detached state.

LV. *Notice of the great Meteor seen on June 29th.* *By* R. Edmonds, *Jun.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE meteor of June 29th is spoken of by all in this neighbourhood who saw it, as by far the most sublime spectacle they have ever beheld.

The weather for the last two or three days had been very sultry, with little or no winds or clouds. On the night of the 29th the sky was cloudless, and the air, in the lower regions of the atmosphere, almost motionless, the vane pointing nearly east. At eleven o'clock, while walking towards the west, in an open place near this town, a bright light suddenly shone around me. I instantly turned to my left, and at S.E. by S., at an elevation of between 30° and 40°, I beheld an intensely white ball of fire, nearly as large as the meridian moon, and tapering upwards into a tremulous or vibratory tail, eight or ten times longer than its greatest transverse diameter. The meteor *seemed* to have no horizontal motion, but to be descending very slowly and majestically in a perpendicular direction to the earth, at the distance of only a few hundred feet. Words can convey no idea of its sublimity. It was visible for ten or twelve seconds, and then disappeared, hidden, no doubt, behind an eminence about a mile from me. I continued out for half an hour afterwards, but heard no report or sound of any kind proceeding from the meteor. Although the nights are now less dark than in any other part of the year, the eye could scarcely endure the excessive splendour.

A gentleman, who happened to be looking in the direction of the meteor when it first appeared, and who was then travelling on a coach, twenty-five miles east of this place, over a plain bounded only by the horizon, and who consequently saw it a second or more both before and after myself, informed me, that at its first appearance it was in the direction already stated, at an elevation of about 40°; that it appeared very near, and to descend very slowly, without any horizontal motion; that it was first of an intense blue colour, and of the size and form of an egg, with its small end upwards, accompanied with a long luminous train, extending also upwards, and perfectly similar to the train of an ordinary shooting-star; that it soon lost the train, and became a vivid white ball of fire; that it then seemed to burst, or to expand into a roundish but irregular form of a reddish hue, four or five feet in diameter; and finally disappeared, as if it had fallen to the earth, within the limits of the horizon. The gentleman with-

drew his eyes for a second or two from the meteor, to observe the degree of illumination which it diffused, and he saw the wheel-tracks and little stones on the road, and the country around, as clearly as they could be seen by daylight.

I consider the light shed by the phænomenon over this neighbourhood equal to that of a cloudless noon in December.

From the above description, I conclude that the meteor must first have appeared in the S.E. by S., and have moved towards that point until it sank beneath the horizon; its apparent increase of size, and its redness on approaching the horizon, being explicable on the same principle that the moon seems so very large and red when near the horizon, compared with her appearance when southing.

I find that the meteor has been seen in France.

I am, &c.,

Redruth, July 14th, 1832. Rd. Edmonds, Jun.

LVI. *Notice of the Meteor of June 29th; and Inquiries relative to certain Points in Magneto-Electricity. By Mr.* John Prideaux.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE great meteor which appeared on the 29th ultimo, having been very differently represented from different quarters, and having excited attention through a wide range, I have collected as many particulars as could be gained, from a considerable number of observers in our own neighbourhood, and from the crews of vessels who saw more or less of it in their passage hither both from the east and the west. These particulars are transmitted to you, in the hope, that, making your Journal the focus, we may obtain a complete account of it; particularly of its descent, which must have been far to the southward, probably in the Bay of Biscay, if it cleared the French coast.

On the day named, Friday the 29th ult., a large star appeared near the zenith, having extreme brilliancy, its light resembling that of lime under the oxy-hydrogen blowpipe. It passed quickly southward, enlarging as it proceeded, to a circular disc approaching the full moon in size, and the sun in splendour, until within about 5° of the horizon, when it appeared to expand till lost by extenuation, without sound. Its course was marked by a continuous train of light, dilating with its expansion, and generally about five or six times its diameter. The entire form of this train is described as pyra-

midal, or rather as resembling that of a long and narrow paper kite, with its tail completely inverted, so that the end of the tail was uppermost. The time of its appearance was probably five or six seconds, though some persons estimate it to have been much longer. Many persons assert they saw it fall into the sea, with the production of vapour, bubbling, and a hissing noise. Three of these I have met with: one describing it as in Hamoaze; another just outside the Breakwater; and the third near the Lizard Point;—a range of fifty miles. And as each declares it fell quite near him, their account is evidently referrible either to alarm or to imagination.

Here, and by vessels to the eastward, the point of its approach to the horizon seems to have been observed nearly due south, some giving it easterly, some westerly declination. But none of those who saw it from about the Lizard, attribute to it any easterly declination. In such observations, casually made, and without instruments, we can put no confidence as to accuracy; but being generally obtained from sailors, they are, I think, enough to justify the inference, that it was at a considerable distance to the southward. The most complete view of it that has been reported to me, occurred to Captain Tozer, of the Navy, which has served to correct the exaggerated statements of others, whose alarm or imagination was excited on witnessing only the latter part of its course.

The Falmouth Packet newspaper says it was seen from the Scilly Islands to Melksham (Wilts). I have traced its appearance from Buckfastleigh by land, and east of the Start by sea, to the Lizard Point; and, on the authority of Mr. Harris, northward to Barnstaple. You will probably find accounts of its having been observed much further in every direction.

Its greatest splendour seems to have been at sea, where many were obliged to cover their eyes with their hands; and one man, east of the Start, thought his ship was on fire.

Yours, &c.,

Plymouth, July 9th, 1832. JOHN PRIDEAUX.

P.S. There is another subject on which I am still more desirous of information.

It is stated, at sections 99, 100, 121 of Dr. Faraday's Experimental Researches, &c., just published in the Phil. Trans. Roy. Soc., that If the marked pole of a magnet be placed above a copper disc (fig. 15, 27), or the unmarked pole below it, or both, and the disc be rotated screw fashion (or with the sun, as it is expressed by sailors), currents of positive electricity set off from the central parts in the general direction of the radii, by the pole, to the parts of the circumference on the

other side of that pole: and these statements are corroborated by others. (Sect. 101, &c.)

But Mr. Sturgeon's plate, in your Number for July (fig. 16, 17), gives this current reversed, the other circumstances being the same; and the current the same, the rotation being reversed.

It is also shown by Dr. Faraday Sect. 220, 222), that a magnet held in the direction of the earth's axis, unmarked pole up, and rotated with the earth, or unscrew, yields positive electricity at its extremities, and negative at the centre; and that a copper cylinder revolving round the magnet produces the same result; and that the rotation being reversed, the electricities are also reversed.

But in a plate of Mr. Sturgeon's very commodious electro-magnetic apparatus (Ann. Phil. N. S. No. 12, p. 359), with an explanation, it appears that a magnet, unmarked* pole up (fig. 1), subjected to positive electricity at the centre (for in a single pair with a liquid the copper pole is so), and negative electricity at the poles, immediately rotates screw or with the sun. And this is confirmed by fig. 8. of the same plate, where a copper cylinder, on the same pole, subjected to positive electricity at the pole, and negative below, rotates unscrew, or with the earth. Now according to the statement above, this rotation produces positive electricity at the pole; and the rotation of the magnet just described determines positive electricity to the equator. In all other cases positive electricity repels positive and attracts negative; but here it produces, in both cases, those motions by which electricity of the same name is brought to the same point, instead of those which, by determining the opposite electrical state there, would have satisfied this attractive property. Even the repulsion produced by the first few turns would seem likely to stop and revert the motion, yet no such thing occurs.

If these cases have cost others of your readers so much perplexity as they have me, a rationale from either Dr. Faraday or Mr. Sturgeon is a desideratum.

To the former gentleman I would also suggest the expediency of creating a few new words, expressive of the various conditions of electro-magnetic circulation. There has seldom occurred a case in which they are more needed; and I need not point out to him geological and mineralogical synonymy as a vocabulary which has a great many to spare.

* In that particular magnet neither pole is marked; but the letter N being affixed to the marked and S to the unmarked pole of a horseshoe magnet on the same plate (fig. 8), there can be no doubt about their meaning in the figure in question.

LVII. *On certain Irregularities in the Vibrations of the Magnetic Needle produced by partial Warmth; and some Remarks on the Electro-Magnetism of the Earth.* By Robert Were Fox*.

IN prosecuting some inquiries relative to the intensity of the terrestrial magnetism, I have been not a little perplexed by the anomalous results which the vibrating needle afforded, especially when it was removed from one station to another; and in order to ascertain the cause of these discrepancies, I instituted a series of experiments, some of which I shall venture briefly to mention.

I had a box made of sheet copper, leaving one side open, which was afterwards covered with glass for the purpose of observing the vibrations of a magnetic needle delicately suspended in it by unspun silk. This box, the glass side excepted, was inclosed in a copper case of much larger dimensions, with sufficient space between them to admit of my surrounding the former with water at any given temperature.

The needle I employed was six inches long, and vibrated as follows:—With water at the temperature of

130°, it made 40 vibra. in 163″. Commenced with an arc of 90°, and ended with one of 28°

85	...	do.	163	do.	90°	do.	30·5°
54	...	do.	163	do.	90	do.	31°

These results appeared to me to be at variance with the prevailing opinions of the influence of temperature on the vibrations of a magnetic needle, and induced me to enter into further investigations of this subject.

I next placed a wooden box, containing a needle ten inches long, on a heated block of granite, a thermometer having been put into the box to ascertain the temperature. At the temperature of

95°, it made 80 vib. in 510″·5. Arc at first 90°, & ended with 24°

72	...	do.	509·5	...	90	...	24
55	...	do.	509	...	90	...	26·5

A box of slate containing a light needle, ten inches long, upon heated granite also. At the temperature of

85°, it made 30 vib. in 176″·5. Arc at first 40°, & ended with 9°

60	...	do.	177	...	... do.	...	13

A box of slate containing a heavier needle, nine inches and a half long, under similar circumstances. At the temperature of

* Communicated by the Author.

120°, it made 80 vib. in 641″. Arc at first 90°, & ended with 16°·5

104	...	do.	641·5	...	do.	...	18
85	...	do.	641·5	...	do.	...	18
52	...	do.	644·25	...	do.	...	34

A warm blanket was then thrown over the slate box in order to communicate warmth to its top and sides, the supporting granite being at 52°.

At the temperature of 60°, it made 80 vibrations in 645″·5. Arc at first 90°, and ended with 38°.

After the needle had been held a short time in the hand, in order to warm it,

It made 80 vibrations in 642″·2. Arc at first 92°, and ended with 22°.

In these experiments it appears that the number of vibrations was mostly rather increased, and the arcs diminished, when the bottom of the box, or the needle only, had its temperature augmented. When merely the sides of the box were warmed, the result was different; and when the heat was applied as uniformly as possible to all parts of the box, the irregularities of the vibrations were less considerable than when the bottom of it only was heated; and merely touching the latter with the hand for a short time frequently produced considerable derangement in the action of the needle. The effects, however, were often so different when all circumstances appeared to be alike, that it seemed desirable to investigate the subject further.

For this purpose I suspended a very slender needle in a copper case, and placed the latter on supports in a basin, into which I poured warm water till it reached the bottom of the case. An extraordinary agitation of the needle then took place, its vibrations sometimes amounting to ten or fifteen degrees on each side of the meridian, occasionally stopping, and then starting again, and frequently shifting its centre of vibration backwards and forwards on either side of zero; and this motion continued more or less, till the water had approximated to the temperature of the room. At first I fancied the agitation of the needle might be owing to electricity, but subsequent observations have induced me to attribute it to currents of air, rapidly rising and descending in the box containing it. These effects were produced by any heated substances put under the needle, and at the distance of several inches, or even a foot, when the heated body was large; but when it was held above the box containing the needle, the influence was comparatively inconsiderable. In the course of these experiments the needle was suspended in close boxes of metal, slate, and

paper, and in every instance it became affected as soon as the warmth had in any degree penetrated through the bottom of the respective boxes. When other substances, such as slender copper wire, paper, &c. were suspended like the needle, they also were agitated by slight changes of temperature; but when the needle was inclosed in an exhausted receiver, it did not appear to be much, if at all, affected by heat. Hence it becomes manifest that the anomalies so often complained of in making experiments on the vibrations of the needle, may probably have arisen from the box having been partially affected by changes of temperature, produced, perhaps, by having been held in the hand, or by some slight change of position affecting the temperature of the box; and when observations are made in the open air, it is evident that the vibrations may be sensibly disturbed by solar heat, cold wind, and other causes. Indeed, I have found from repeated experiments, that when the needle is vibrated in the sun, the arcs become rapidly diminished, and the vibrations consequently increased in number; but in all cases of exposure to warmth, it appears that the vibrations and arcs are very anomalous, depending, no doubt, on the direction in which the excited aërial currents act on, or strike the needle:—hence the discrepancies which occurred in my experiments above stated.

It is therefore obviously important that the magnetic needle should be exposed as little as possible to fluctuations of temperature, and that it should be contained in a box made of wood, or of some other imperfect conductor of heat; and for the same reason there would be an advantage in having the glass doubled. The needle itself should not be too light, and the cylindrical form will least expose it to being disturbed by currents of air. It is, however, evident, that no remedy can be so effectual as exhausting the air, which, when it can be conveniently done, will add much to the value of experiments with the vibrating needle, and render all observations on the compass, in which great accuracy is required, more deserving of confidence*. It might, however, be unnecessary to exhaust the air, if the needle were suspended in a vessel surrounded with water, or nearly so, at a given temperature. Indeed it might sufficiently answer the purpose to have the top and bottom of the vessel or box, furnished with a double metallic

* It seems that my friend W. S. Harris has for some time past been in the habit of using a vibrating magnetic needle suspended in an exhausted receiver; and I have very recently seen his apparatus, which appeared to me to be admirably adapted for making experiments on the terrestrial intensity.

case to contain water, the sides being surrounded with a bad conductor of heat, except a space left to be covered with double glass, for the purpose of observation; for it appears from my experiments that the needle is not much affected by a change of temperature at the sides of the box only.

It has till within a very recent period been generally assumed that the earth's magnetism is owing to a central magnet, notwithstanding the incongruity of many facts with such an hypothesis. Indeed, if we admit the existence of intense heat in the interior of the globe, we have every reason to believe that magnetism cannot exist there; since it appears, that neither the loadstone nor steel can retain it at a high temperature, and that iron at a white heat loses its power of attracting the needle.

The discoveries of Oersted and Seebeck have, however, laid the foundation of juster views of this interesting subject, and many difficulties vanish when the phænomena of the earth's magnetism are referred to the circulation of electrical currents around it. This hypothesis, which was first suggested by Ampère, appeared to me to derive strong confirmation from the stratification of rocks, the arrangement of metallic and other veins, the high temperature which in a greater or less degree prevails under the surface of the earth, and its rotation on its axis, possessing as they seemed to do, many analogies to electro-magnetic, and thermo-electric combinations. I was consequently led to suspect the existence of free electricity in metalliferous veins, and was not disappointed*.

If we take a glance at the map of the world, we perceive that it consists of two grand divisions of land, and two of water, alternating with each other, from east to west. This curious arrangement seems to bear on the point in question, as well as the difference of temperature found generally to exist between the eastern and western sides of great continents; the lines of minimum temperature may possibly coincide with those of no variation; at least this point seems to deserve investigation when opportunities occur.

The direction of the electrical currents under the earth's surface may be greatly diversified; this may be inferred from my experiments on the electricity of metallic veins †. But the facts I have referred to, especially the rotation of our planet from west to east, and the solar rays acting in a contrary direction, would induce us to suppose that the currents, taken col-

* In some lead-mines in Flintshire, where the temperature is low, I could not detect any free electricity. Was this fact owing to their being situated in horizontal strata?

† See Phil. Trans. 1830, p. 400, &c.

lectively, must have a prevailing tendency; and it follows from the direction of the needle, that this tendency, as it respects the positive currents, must be from the east, more or less towards the west.

It appears that the ores themselves, in some instances, possess opposite thermo-electric properties. The sulphurets of lead and of copper, for example, when partially heated in a very moderate degree, yielded positive electricity to the less heated part; whereas in the case of sulphuret of iron, it was yielded from the latter to the former. When two of these ores were respectively placed in contact with each other at different temperatures, the sulphuret of lead was always positive with respect to the other two, whether it was at a higher or lower temperature than they were; and the sulphuret of copper was, when heated, positive with regard to iron pyrites, but negative when the temperature of the latter was the greater. In some instances the nature of the electricity became reversed before the heated ore had entirely cooled; this occurred when lead or copper ore was placed in contact with iron pyrites at an inferior temperature.

These different thermo-electric properties of metallic substances seem to throw some light on the cause of opposite currents in mineral veins, and are, perhaps, connected with the periodical variation of the needle.

Several observations have been made in the mines of Cornwall on the intensity of the earth's magnetism, from which it is to be inferred, that if at the greatest accessible depths it differ at all from the intensity at the surface, the difference is very inconsiderable, and that therefore the principal source, or cause of the terrestrial magnetism, must be far removed from us, so far indeed as to require powerful electrical currents to produce the effects observable at the surface.

LVIII. *Remarks on the Structure and Affinities of* Cephalotus. *By* Robert Brown, *Esq. F.R.S. &c.* *

In the Botanical Appendix of Captain Flinders's Voyage to Terra Australis, a figure and description of *Cephalotus follicularis* are given, in some respects more complete than those of M. Labillardière, by whom this remarkable plant, a native of the south-west coast of New Holland, was first published. Both accounts, however, are equally imperfect with regard to the fruit; and my principal object in the present communication is to supply that deficiency.

My earliest knowledge of the ripe fruit of Cephalotus was

* Communicated by the Author.

obtained from a single specimen, sent in 1815 by M. Lechenault, who had found the plant in February 1803 near the shores of King George's Sound, where I had gathered it in a less advanced state in the beginning of January 1802.

I have, however, more recently, received numerous specimens with ripe seeds from Mr. William Baxter, who collected them also at King George's Sound in 1829.

Cephalotus was introduced in 1823 from the same place by Capt. King, into His Majesty's Botanic Garden at Kew, where it flowered repeatedly, and ripened seeds from which several plants have been raised. A figure of one of these with expanded flowers, but still without fruit, has lately been published by Dr. Hooker in the Botanical Magazine; and a plant brought also from King George's Sound in 1829 by Mr. William Baxter is now in flower in Mr. Knight's nursery.

The following account of the ripe fruit will serve as a supplement to the description of the plant which I have given in the work referred to.

Akenia membranacea, insecta parva alis conniventibus quodammodò referentia, perianthio parùm aucto staminibusque persistentibus cincta, iisque sesquilongiora, ferè distincta, ipsâ basi, ubi receptaculo communi inserta, post separationem intùs aperta ibique è membranâ simplici crassiusculâ imberbi nitente formata; suprà clausa et è duplici membranâ conflata; harum exterior densè barbata, pilis longis, strictis, acutis, deflexis, stylo persistenti brevi arctè reflexo rostrata: membrana seu lamella interior tenuis, intùs quandoque dehiscens.

Semen unicum (rarissimè duo), basi cavitatis membranæ interioris insertum, oblongo-ovale, teres, funiculo umbilicali brevi juxta basin affixum. *Integumentum* duplex: *Testa* membranacea laxiuscula, *raphe* tenui laterali et apice *chalazâ* parvâ insignita: *Membrana interior* tenuis separabilis. *Albumen* semini conforme, album, carnosum, subfriabile, è materiâ oleosâ cum granulis minutis mixtâ constans.

Embryo parvus, in basi axeos albuminis, teretiusculus, albus, rectus, albumine 4—5ies brevior. *Cotyledones* breves, plano-convexæ. *Radicula* teres, basin seminis attingens.

Receptaculum commune fructûs: tuberculum centrale, parvum, brevissimum, subcylindraceum, cujus lateribus bases apertæ akeniorum adnatæ sunt, apice convexiusculo barbato.

From this description, especially of the embryo, it is evident that Cephalotus must be removed from Rosaceæ, to which it had been referred by M. Labillardière; and also, though not with much confidence, in the account which I published in 1814. M. de Jussieu, indeed, in 1818, proposed to exclude

it from Rosaceæ and append it to Crassulaceæ; and the structure of the seed, as well as of the folliculi or akenia, and even their insertion on the minute central receptacle or axis, may seem to confirm the correctness of this approximation.

Cephalotus, however, still appears to me sufficiently remote from every natural order at present established, to entitle it (like Philydrum * and Brunonia †), now that its structure is completely known, to rank as a distinct family which may be called CEPHALOTEÆ, and which may be placed between Crassulaceæ and Francoaceæ; differing from both in being apetalous, in the valvate æstivation of the perianthium, and in many characters of inferior importance: from Crassulaceæ also in its minute embryo and more copious albumen; and from Francoaceæ in the absence of barren stamina and in the pistilla being monospermous and apparently distinct.

The most striking peculiarity of Cephalotus consists in the conversion of a portion of its radical leaves into Ascidia or pitchers. But as ascidia in all cases are manifestly formed from or belong to leaves, and as the various parts of the flower in Phænogamous plants are now generally regarded as modifications of the same organs, the question is naturally suggested, how far the form and arrangement of the parts of fructification agree in those plants whose leaves are capable of producing ascidia or pitchers. The four principal, and indeed the only genera in which pitchers occur, are Nepenthes, Cephalotus, Sarracenia, and Dischidia, and the few other somewhat analogous cases, consisting of the conversion of bracteæ or floral leaves into open cuculli, are found in Marcgravia and two other genera of the same natural family.

The only thing common to all these plants is, that they are Dicotyledonous.

It may also be remarked, that in those genera in which the Ascidia have an operculum, namely Nepenthes, Cephalotus, and Sarracenia, they exist in every known species of each genus, and the structure of these genera is so peculiar that they form three distinct natural families; while in Dischidia, whose pitchers are formed without opercula, these organs are neither found in every species of the genus, nor in any other genus of the extensive natural order to which it belongs.

The striking resemblance in most points of the Ascidia of Cephalotus to those of Nepenthes, leads to a comparison in the first place of these two genera. But although both are apetalous, and in the parts of the flower deviate from the quinary or prevailing number in Dicotyledones, yet they differ

* Flinders's Voyage, vol. ii. p. 578.
† Transact. Linn. Soc. vol. xii. p. 132.

in so many other important characters that they cannot be considered as nearly related.

The place of Nepenthes in the natural series I have long since*, in my account of Rafflesia, suggested to be near Aristolochiæ or Asarinæ, without, however, intending to include it in that family.

This approximation was adopted by M. Ad. Brongniart, who, however, went further, having absolutely referred Nepenthes to Cytineæ.

The union of plants so utterly unlike in appearance and œconomy, and so different, it may be added, in many of their most important characters, seems to have been generally regarded as somewhat paradoxical; and accordingly Professor Link, in 1829, has established Nepenthes as a section or tribe of Aristolochiæ, and Dr. Bartling and Mr. Lindley, in 1830, have considered it as forming a distinct natural family.

To the numerous and obvious distinctions between Cytineæ and Nepenthes may be added the no less important differences in their internal structure. For while Cytineæ, like most, perhaps all, other plants parasitical on roots, are destitute of spiral vessels, Nepenthes exhibits these vessels in the greatest degree of development and abundance, and also produces them in parts in which they are hardly to be met with in any other dicotyledonous plant.

Thus, in addition to the dense circle or stratum of spiral vessels existing in the stem between the outer parenchyma and the wood, they are found also singly or scattered in the pith, in the loose parenchyma situated between the wood and the bark, if it may be so called, even in the fibres of the root, and everywhere in the substance of the leaves, the pitchers, calyx and capsules. And between these solitary or scattered spiral vessels, which are often of considerable length, and those forming the stratum or circle externally bounding the wood and existing in the veins of the leaves, no essential difference in structure will I believe be found. In these points there is little resemblance between Nepenthes and Cephalotus, in the internal structure of which last there is nothing unusual.

Between the parts of fructification of Nepenthes or Cephalotus and Sarracenia, there is still less analogy, and it is obviously unnecessary to compare in this respect any of these genera with Dischidia.

September 25th, 1832.

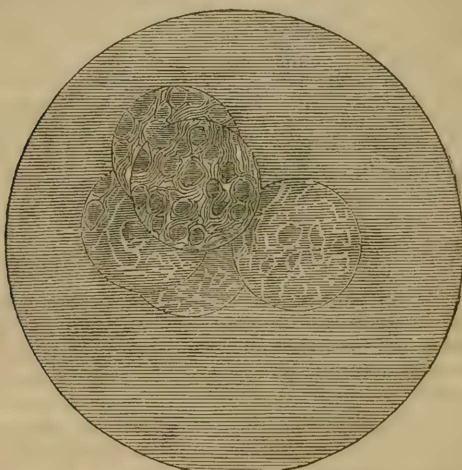
* Transact. Linn. Soc. vol. xiii. p. 219.

LIX. *Account of an Experiment in which part of the interior of the Eye is exhibited by Reflection in the Eye-glass of a Telescope*.*

THE account of Dr. Purkinje's experiment in the September Number of the Philosophical Magazine has induced me to record the following fact, which bears some analogy to it, and which has been too often remarked and too carefully examined to admit of any doubt as to the circumstances.

Being in the habit of occasionally looking at the sun through a very fine and powerful achromatic telescope, I have frequently been unable to distinguish the spots, being perplexed by what appeared the reflection of some part of my own eye, interposed between it and the sun; and this, whether the eye approached the telescope as closely as possible, or was withdrawn to some little distance.

I could not believe that it arose from the eye becoming dazzled by the light, because it was capable of bearing a much intenser application and far stronger glare without fatigue, and of looking immediately from the object in question to the white paper on which I delineated it.



On reading the passage in Sir D. Brewster's communication, as above, p. 173, I immediately recollected, that though three differently coloured glasses had been used for the sun, it was

* Communicated by the Rev. T. J. Hussey.

only with one transmitting a reddish brown orange tint that I had been annoyed by this reflection; to render the description of which more intelligible, I have made the sketch from several distinct observations taken for the purpose. The *number* of the reflections never varied; there were *five* apparent, one behind the other, the one in front, and the upper part of the second, being the brightest, the others growing gradually more indistinct: they were perfectly circular, except at the upper edge to the left, where the outline was rather broken by two dark spots. Instead of appearing *dark relieved by the light as a ground*, they were exactly *vice versá*, looking like a bright film composed of silver ramifications*, the dark spots seeming like holes with strongly illuminated margins; and the *pattern* of the ramifications *never changing*.

It was not till I had been induced to scrutinize the appearance more attentively, in order if possible to ascertain what it was I really did see, that I perceived in advance of the five bright circles a sixth, like a small dusky cloud†. I could never distinguish any ramifications upon it; indeed the very outline was so slightly defined that I did not attempt to represent it on paper; but there it always was, only disappearing when I looked against the side of the tube: this the bright ones did not do immediately,—they faded gradually away. These reflections were most visible when the whole field was filled with the sun; when only a small part of the disc was in it, they disappeared, or nearly so.

The orange-brown tint always shone through the silvery films, or rather between the ramifications of the network.

A. M. H.

Hayes, Sept. 1832.

LX. *Intelligence and Miscellaneous Articles.*

ON THE TRUE SOURCE OF THE AMNIOTIC ACID OF VAUQUELIN (ALLANTOIC ACID OF LASSAIGNE): AND ON THE IMPORTANCE OF OBTAINING COMPARATIVE ANALYSES OF THE ALLANTOIC FLUID, AND THE URINE OF THE YOUNG ANIMAL AFTER BIRTH.

IN Dr. Thomson's Inorganic Chemistry (vol. ii. p. 167), the Amniotic acid of Vauquelin and Buniva is described under the appellation of *Allantoic acid*, upon the authority of Lassaigne. The chemist last named, it appears, examined, three successive times,

* The dark outline of course did not appear; it is merely used in the drawing because the circles could not otherwise be defined. The drawing supposes the whole field of the telescope filled with the sun.

† The bright ones showing through it.

the fluids contained in the amnios and allantois of the cow, and always found the acid in question in the fluid of the latter membrane, but never in that of the former. He concluded from these results, that the allantoic fluid had been given for analysis to Vauquelin and Buniva, instead of the amniotic, and changed the name of the acid accordingly. At present, therefore, our knowledge of the true source of this acid rests upon the authority of Lassaigne alone. But it is always desirable that new facts should not rest upon the testimony of a single observer, however deservedly high may be his reputation; and as the existence of strong independent, though partial evidence, in favour of Lassaigne's conclusions, has apparently been overlooked by that chemist (as well as by Dr. Thomson, though published in the journal formerly conducted by himself), it may be useful to draw the attention of chemists to the subject. This seems the more requisite, because the authority of Dr. Prout, by whom the confirmatory evidence has been furnished, is so valuable upon a point of this nature, on account of his minute acquaintance with the animal fluids, and his practical skill in their examination; and because, also, the authors of several of our systematic treatises on Chemistry (Dr. Henry and Dr. Turner for example,) have not noticed Lassaigne's revision of the subject, but have retained the amniotic acid, as such, in the sections on animal chemistry of their respective works.

In 1815 Dr. Prout published, in the Annals of Philosophy (First Series, vol. v. p. 416), an account of his examination of the *liquor amnii* of a cow. His attention, he states, in this examination, was particularly directed to the principle found in that fluid by Vauquelin and Buniva, and called by them *amniotic acid*, but that he could not, however, discover the least traces of a similar principle. This negative result, therefore, confirms those of Lassaigne, who, as above stated, could never find the acid in the fluid of the amnios.

Further evidence, however, is derivable from Dr. Prout's paper, in confirmation of Lassaigne's opinion that the fluid examined by Vauquelin and Buniva was truly that of the allantois. Dr. Prout states that the fluid he examined differed very considerably from that described by them, in its sensible qualities, as well as in its chemical ones; and although he ascribes this dissimilarity to the circumstance that his was taken from an animal slaughtered in an early period of her gestation, while theirs, most likely, he observes, was procured at the full period, it is evident that the existence of differences so great is far better explained, by the supposition, that Vauquelin and Buniva in reality examined a different fluid, or at least one which did not wholly consist of *liquor amnii*. A comparison of the results obtained by Vauquelin and Buniva with those of Dr. Prout, tends rather to indicate that the fluid examined by them consisted of the mixed fluids of the amnios and allantois, than that it was the allantoic fluid alone, as supposed by Lassaigne. Thus, the *liquor amnii* examined by Dr. P., as well as the fluid examined by the former chemists, gave a copious white precipitate with muriate of barytes; both contained an organic substance soluble in alcohol, and both also yielded a substance pre-

cipitable by alcohol. The mixture of the fluids might easily occur from the rupture of the membranes. I give this opinion, however, without being aware how far the examination of the allantoic fluid by Lassaigne, (except as to its containing the acid,) may agree with that of the supposed amniotic fluid by Vauquelin and Buniva.

Again: the agreement between the results of Dr. Prout's analysis of the *liquor amnii* of the cow, and those of Vauquelin and Buniva's analysis of the corresponding fluid of the human subject (respecting the origin of which, of course, no mistake could have occurred), may be adduced in support of Lassaigne's opinion. According to these analyses, the two fluids agree in the following circumstances (in which, at the same time, they both differ from the alleged amniotic fluid of the cow examined by Vauquelin and Buniva): In colour, smell, and taste, they evidently belong to the same class of fluids; their differences, in those respects, being no greater than always exist between the corresponding products of animals generically different; while they agree in containing minute floating particles apparently caseous, in foaming when shaken, in partial coagulation by heat, in the action of acids, and in containing albumen and salts of soda: Dr. Prout, likewise, found sugar of milk in the fluid of the cow; while Vauquelin and Buniva observed that alcohol threw down from that of the human subject a light precipitate, which, when dry, became brittle and transparent like glue,—characters which would be assumed by slightly impure sugar of milk, in this mode of operating.

Another corroboration of Lassaigne's results may be deduced from the situation and functions, respectively, of the amnios and the allantois. The latter, receiving the urine of the fœtus, would more probably contain a fluid of an acid nature than the former; indeed, it would seem that the contents of the allantois could not but be acid; while there is no apparent reason why (in animals provided with the latter membrane) the fluid of the amnios should have any considerable degree of acidity.

The analogies (which are considerable) connecting the allantoic with the uric acid, seem further to corroborate the same view of the subject: the urine of the cow, like that of other herbivorous *Mammalia*, does not contain uric, but benzoic acid; but the urine of the fœtal calf, however, we might reasonably expect, since the nourishment it receives is altogether animalized, (though produced from the vegetable food of the cow,) would contain some principle analogous to uric acid. If this notion be correct, we should expect to find allantoic acid in the urine of the calf while it receives nourishment by sucking, and perhaps that the benzoic acid (since the milk is devoid of that principle) would not appear until it begins to graze.

It may be requisite here to anticipate an objection which might arise, on the ground that the urine of the *Mammalia* taking animal food exclusively, does not contain uric acid. That fact might be supposed to invalidate the inference, that the urine of the fœtal and of the sucking calf (since the animal, in those states, receives animalized nutriment alone,) ought to contain some principle analogous to uric acid. But among the *Mammalia*, those species only appear to secrete uric acid

which are omnivorous, or at least such as take food of a mixed nature; and this exactly accords with the nature of the calf's nutriment, as derived from the slightly animalized fluids of an exclusively herbivorous animal, either as supplied to the fœtus, or in the form of milk.

If the above reasoning be correct, and if the allantoic acid be really derived from the fœtal urine, the subject appears to acquire a degree of importance which has not hitherto attached to it. It would be very desirable to ascertain whether the acid is also contained in the urine of the young animal after birth; and a series of comparative experiments on the allantoic fluid, and on the urine of the young animal, while sucking only, while it both sucks and grazes, and after lactation has entirely ceased, in all the *Mammalia* in which the allantois exists, might lead to important results, relative to the functions of the fœtal urinary system, and perhaps also to the quality and process of formation of the fœtal blood. It may be remarked, in relation to this subject, that great additional benefits would be conferred upon physiology, if the attention of the Committee of Science of the Zoological Society,—the investigation of the comparative anatomy of various animals by whom, has already thrown so much light on the relations of their minute anatomical structure to their respective stations in nature, as well as on their physiology in general, —were extended to the performance of experiments on the contents of the animal fluids. The cow, the mare, and the ewe, in all which the allantois is found, are readily accessible; but the collection of the Zoological Society consists, principally, of animals, which not being objects of rural or commercial œconomy, cannot often furnish subjects for investigation by the animal chemist, but peculiar facilities for researches on which are presented by that Society's establishment.

With respect to the source of what has hitherto been called amniotic acid, it may be said, perhaps, that the repeated experiments of Lassaigne are amply sufficient to determine the point; but as it would appear, from the silence on the subject of Mr. Brande, Dr. Henry, and Dr. Turner, that his results are not generally received or attended to by chemists in this country, the foregoing remarks upon it may not be superfluous.

Sept. 22, 1832. E. W. B.

OBSERVATIONS OF THE TRANSIT OF MERCURY, ON MAY 5, 1832, MADE AT HULL, BY MR. J. D. SOLLITT.

External ingress, or beginning of the transit, . .	$4^d\ 20^h\ 59^m\ 1^s$
Internal ingress, ...	4 21 2 21

At 22^h it became thick and rainy, and remained so during the rest of the day.

The above observations are for Mean Time at Hull.

Latitude of the place of observation, ...	53° 45′ 57″ N.
Longitude in time	1 21 W.

Hull, June 26th, 1832. J. D. S.

AN EPHEMERIS OF THE STARS PROPER TO BE OBSERVED WITH *MARS*, AT THE ENSUING OPPOSITION OF THAT PLANET.

[We transfer the following to our pages, from the Supplement to No. 11. of the Monthly Notices of the Astronomical Society, on account of the importance of the object contemplated by the Astronomer Royal at the Cape, and in order to give additional publicity to the Ephemeris itself. The latter is now given from October 11th to November 7th; and the corresponding portions for November and December will appear in our Numbers for those months.]

Previous to Mr. Henderson's departure for the Observatory at the Cape of Good Hope, (to which he has recently been appointed Astronomer, in the room of the late Rev. Fearon Fallows) *, he expressed a wish that a selection might be made of such stars as would be proper and convenient to be observed with *Mars*, at his ensuing opposition in November next; with a view to the determination of the parallax of that planet; and that a list of the same should be circulated amongst different astronomers in various parts of the world, for the purpose of obtaining corresponding observations.

Mr. Sheepshanks having furnished the apparent places of *Mars* (together with the semidiameter and horizontal parallax) for each day during the requisite period, Mr. Baily selected the stars agreeably to Mr. Henderson's wishes: and the Council of this Society, desirous of promoting, as much as lies in their power, an object which, if actively and properly followed up, may be attended with much advantage to the science of astronomy, have caused the same to be printed and circulated.

The positions of *Mars* are the *apparent* geocentric places (corrected for aberration) at mean midnight at *Berlin;* deduced from the Berlin Ephemeris, using 5th differences in the computation. The positions of the stars also are their *apparent* places on the day of transit: Mr. Sheepshanks having furnished the daily corrections for precession, aberration, and nutation. These stars are selected in such manner that there may always be a sufficient interval of time between the transit of the star and the planet, to enable the observer to read off the divisions of the circle or micrometer; except in a few cases when they are both in the field of the telescope at the same time, or so nearly on the same parallel that one setting of the instrument will be sufficient for both observations, with the aid of a micrometer.

Mr. Henderson requests that, when both limbs of *Mars* cannot be conveniently observed on the same day, the *northern* limb should be observed on the *odd* days, and the *southern* limb on the *even* days of the month: as a guide to the observer, this is denoted by the letters N and S inserted in the column of magnitudes. Also, that the transit of the *second* limb should be observed *prior* to the day of opposition, and the transit of the *first* limb *after* that day: this is denoted by the figures 1 and 2 annexed to *Mars*.

* See our Number for September, pp. 237, 242.—EDIT.

Aldebaran should be observed on every night when the planet is observed.

Those astronomers, who are possessed of good equatorial instruments, may take *repeated* measures of the difference of declination between the selected star and the planet on the same night: noting, however, the times at which the observations were made.

The Ephemeris is extended from October 11th to December 25th, for the purpose of including the stationary points of *Mars*, both in right ascension and declination.

The star (38) *Arietis* is to be found in Piazzi's catalogue: the small stars (*a*) (*b*) (*c*) are taken from Lalande's *Histoire Céleste*, page 33. The star (*b*) is the brightest, the most northerly, and the second of two stars that are distant from each other about 2′ in declination; and *between* which *Mars* will pass on November 17th. For this, and for two or three other proximate stars, the wire micrometer might be advantageously used in determining the difference of declination. The places of the larger stars are taken from the catalogue of this Society, and the constants there given are used in the reductions.

The following are the assumed mean places, on January 1, 1832, of the 10 stars selected for the comparisons; viz.

Star.	Mag.	Mean Æ.	Mean D. *North.*
		h m s	° ′ ″
(38) Arietis	8	3 11 12,37	19 53 50,48
65 ———	6	14 44,87	20 12 9,87
* Tauri (*a*)	9	29 15,55	20 21 45,62
F¹ ——	6·7	32 38,01	19 9 23,82
32 ——	6	46 56,77	21 59 18,00
* —— (*b*)	8	47 20,25	20 49 44,88
A¹ ——	5	54 46,12	21 36 55,94
51 ——	7	4 8 26,98	21 9 43,42
53 ——	6·7	9 32,03	20 43 44,60
* —— (*c*)	8	16 21,26	21 4 53,84

Other quantities, used in the computations, are the following: viz.

Sun's horizontal parallax $= 8'',578$
Mean semidiameter of Mars $= 4\ ,790$
Constant of aberration $= 493^s,2$

Berlin, East of Paris $= 44^m\ 12^s,6$
——— of Greenwich $= 53\ \ 34\ ,1$

⁎ Mr. Henderson was also desirous that some stars should be selected for observing the parallax of *Mars* in *right ascension;* agreeably to the method pointed out by Lalande, in his *Astronomie*, vol. ii. page 281: since *Mars* will be favourably situated, at the ensuing opposition, for such observations in the northern hemisphere. But there are no stars, near the path of the planet at that time, of sufficient magnitude for such purpose.

1832.	Stars.	Mag.	Apparent Place. Right Ascens.			Declin. *North.*			Semidiameter. In time.	In arc.	Hor. Par.
			h	m	s	°	′	″	s	″	″
Oct. 11	53 Tauri	6.7	4	9	34,69	20	43	48,0			
	* —— (c)	8		16	23,90	21	4	56,7			
	Mars²	N		22	55,23	20	36	55,1	0·586	8,22	14,71
12	53 Tauri	6.7	4	9	34,71	20	43	48,1			
	* —— (c)	8		16	23,92	21	4	56,8			
	Mars²	S		22	59,00	20	39	22,5	·590	8,28	14,82
13	53 Tauri	6.7	4	9	34,74	20	43	48,1			
	* —— (c)	8		16	23,95	21	4	56.8			
	Mars²	N		22	59,07	20	41	44,0	·594	8,34	14,92
14	53 Tauri	6.7	4	9	34,76	20	43	48,2			
	* —— (c)	8		16	23,97	21	4	56,8			
	Mars²	S		22	55,40	20	43	59,6	·599	8,40	15,03
15	53 Tauri	6.7	4	9	34,78	20	43	48,2			
	* —— (c)	8		16	23,99	21	4	56,9			
	Mars²	N		22	47,94	20	46	9,3	·603	8,46	15,14
16	53 Tauri	6.7	4	9	34,81	20	43	48,3			
	* —— (c)	8		16	24,02	21	4	57,0			
	Mars²	S		22	36,66	20	48	12,9	·607	8,51	15,24
17	53 Tauri	6.7	4	9	34,83	20	43	48,3			
	* —— (c)	8		16	24,04	21	4	57,0			
	Mars²	N		22	21,55	20	50	10,4	·612	8,57	15,34
18	53 Tauri	6.7	4	9	34,85	20	43	48,4			
	* —— (c)	8		16	24,07	21	4	57,1			
	Mars²	S		22	2,59	20	52	1,7	·616	8,63	15,44
19	53 Tauri	6.7	4	9	34,87	20	43	48,4			
	* —— (c)	8		16	24,09	21	4	57,1			
	Mars²	N		21	39,77	20	53	46,2	·620	8,69	15,54
20	53 Tauri	6.7	4	9	34,90	20	43	48,5			
	* —— (c)	8		16	24,11	21	4	57,2			
	Mars²	S		21	13,09	20	55	24,0	·624	8,74	15,63
21	53 Tauri	6.7	4	9	34,92	20	43	48,5			
	* —— (c)	8		16	24,14	21	4	57,2			
	Mars²	N		20	42,55	20	56	55,0	·628	8,79	15,73
22	53 Tauri	6.7	4	9	34,94	20	43	48,6			
	* —— (c)	8		16	24,16	21	4	57,3			
	Mars²	S		20	8,18	20	58	19,1	·632	8,84	15,83
23	53 Tauri	6.7	4	9	34,96	20	43	48,6			
	* —— (c)	8		16	24,18	21	4	57,3			
	Mars²	N		19	30,01	20	59	36,3	·636	8,90	15,93
24	53 Tauri	6.7	4	9	34,98	20	43	48,7			
	* —— (c)	8		16	24,20	21	4	57,4			
	Mars²	S		18	48,06	21	0	46,3	·640	8,95	16,02
25	53 Tauri	6.7	4	9	35,00	20	43	48,7			
	* —— (c)	8		16	24,22	21	4	57,4			
	Mars²	N		18	2,39	21	1	49,1	·643	9,00	16,12

1832.	Stars.	Mag.	Apparent Place.		Semidiameter.		Hor. Par.
			Right Ascens.	Declin. *North.*	In time.	In arc.	
			h m s	° ′ ″	s	″	″
Oct. 26	51 Tauri	6.7	4 8 29,99	21 9 47,6			
	* —— (*c*)	8	16 24,25	21 4 57,4			
	Mars[2]	S	17 13,06	22 2 44,4	0·647	9,05	16,20
27	51 Tauri	6.7	4 8 30,01	21 9 47,6			
	Mars[2]	N	16 20,15	21 3 32,1	·650	9,10	16,28
	* Tauri (*c*)	8	16 24,27	21 4 57,5			
28	51 Tauri	6.7	4 8 30,03	21 9 47,6			
	Mars[2]	S	15 23,73	21 4 11,9	·653	9,14	16,35
	* Tauri (*c*)	8	16 24,29	21 4 57,5			
29	51 Tauri	6.7	4 8 30,05	21 9 47,7			
	Mars[2]	N	14 23,89	21 4 43,9	·656	9,18	16,42
	* Tauri (*c*)	8	16 24,31	21 4 57,6			
30	51 Tauri	6.7	4 8 30,07	21 9 47,8			
	Mars[2]	S	13 20,74	21 5 7,8	·659	9,22	16,49
	* Tauri (*c*)	8	16 24,33	21 4 57,6			
31	51 Tauri	6.7	4 8 30,09	21 9 47,8			
	Mars[2]	N	12 14,38	21 5 23,8	·661	9,26	16,56
Nov. 1	A[1] Tauri	5	3 54 49,28	21 37 1,3			
	Mars[2]	N	4 11 4,93	21 5 31,7	·664	9,29	16,62
2	A[1] Tauri	5	3 54 49,30	21 37 1,4			
	Mars[2]	S	4 9 52,50	21 5 31,4	·666	9,32	16,67
3	A[1] Tauri	5	3 54 49,31	21 37 1,4			
	Mars[2]	N	4 8 37,25	21 5 22,9	·668	9,35	16,72
4	A[1] Tauri	5	3 54 49,33	21 37 1,5			
	Mars[2]	S	4 7 19,30	21 5 6,2	·670	9,38	16,77
5	A[1] Tauri	5	3 54 49,35	21 37 1,5			
	Mars[2]	N	4 5 58,81	21 4 41,2	·671	9,41	16,81
6	A[1] Tauri	5	3 54 49,36	21 37 1,6			
	Mars[2]	S	4 4 35,92	21 4 7,8	·673	9,43	16,84
7	A[1] Tauri	5	3 54 49,38	21 37 1,6			
	Mars[2]	N	4 3 10,81	21 3 26,3	·674	9,45	16,87
	53 Tauri	6.7	9 35,25	20 43 49,2			

SEPARATION OF THE OXIDES OF LEAD AND BISMUTH.

BY M. LIEBIG.

When nitrate of lead or of bismuth is boiled with carbonate of lime, magnesia, or barytes, these salts are decomposed, and the oxides are so completely precipitated that hydrosulphuret of ammonia shows no traces of them in the solution. Carbonate of lime, when added to a cold solution of these metals, precipitates only the oxide of bismuth.

Several methods have been proposed for separating the lead which is contained in the bismuth of commerce; but carbonate of lime, used in the mode now stated, is preferable to them.—*Ann. de Chim. et de Phys.* tom. xlviii. p. 290.

OCCULTATION OF SATURN, OBSERVED AT GENEVA.

This phænomenon, which took place on the 8th of May, was observed by M. Gautier with the Dollond's telescope described in the last Number of Phil. Mag. p. 246. The same clock was used, but it was now only three seconds and a half fast.

	h	m	s
Entrance of ring behind the moon	9	44	20
First contact of the planet's disc......	9	44	34
End of the planet's entrance	9	45	28
End of the ring's entrance...........	9	45	58·5
End of the emersion	10	47	10

At the end of the emersion, M. Gautier observed that the light of Saturn was then *singularly pale* and of a *grayish-green colour*, "from the effect of the lustre of the illuminated limb from which the planet emerged."—*Bibl. Univ.* April 1832.

NEW PROCESS FOR OBTAINING MORPHIA.

M. Ant. Galvani has proposed a new method of obtaining morphia directly from opium, free from narcotine:—Evaporate to the consistence of an extract a spirituous solution of opium; then, by successive solutions and filtrations, separate all the resinous matter of the extract, which separates the narcotine from the morphia: long ebullition with calcined magnesia,—a series of filtrations, and washings and dryings, yield very pure morphia, free from narcotine. When the resinous matter is dissolved in dilute sulphuric acid, and the solution decomposed by potash, the narcotine is precipitated, which is purified by a fresh solution in sulphuric acid and precipitation by ammonia, and this often, after filtration, washing and redissolving in alcohol of 0·903, crystallizes. A pound of opium yielded by this process 8 drachms of perfectly pure white crystallized morphia.—*Ann. de Chim. et de Phys.* tom. xlviii. p. 297.

LUNAR OCCULTATIONS FOR OCTOBER.

Occultations of Planets and fixed Stars by the Moon, in October 1832. Computed for Greenwich, by Thomas Henderson, *Esq.; and circulated by the Astronomical Society.*

1832.	Stars' Names.	Magnitude.	Ast. Soc. Cat. No.	Immersions.				Emersions.			
				Sidereal time.	Mean solar time.	Angle from North Pole.	Angle from Vertex.	Sidereal time.	Mean solar time.	Angle from North Pole.	Angle from Vertex.
				h m	h m	°	°	h m	h m	°	°
Oct. 4	30 *r* Capric.	6	2520	19 57	7 3	75	63	21 8*	8 14	319	319
13	104 *m* Tauri	5	592	3 36	14 6	65	43	4 34	15 4	323	317
14	χ^2 Orionis..	6	731	22 21	8 48	73	37	23 6	9 33	307	268
	χ^3 Orionis..	5	750	2 2	12 27	79	38	3 4	13 29	303	267
15	ζ Geminor .	4	872	1 57	12 19	102	60	2 56	13 17	268	226
18	34 Leonis...	6	1214	6 56	17 6	104	68	7 56	18 5	220	193
31	20 Capricor.	6	2484	21 12	6 32	99	103	22 33	7 52	297	313

* Star on meridian at emersion.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Giddy at Penzance, and Mr. Veall at Boston.

Days of Month, 1832.	Barometer.					Thermometer.					Wind.			Rain.		
	London.		Penzance.		Boston	London.		Penzance.		Bost.	Lond.	Penz.	Bost.	Lond.	Penz.	Bost.
	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	8½ A.M.						
Aug. 1	30·095	29·974	29·904	29·842	29·57	78	55	63	58	60·5	E.	SE.	calm	0·27	0·175	...
2	29·872	29·844	29·872	29·848	29·37	85	58	68	57	62	S.	NW.	E.	·45	...	0·25
☽ 3	29·991	29·927	29·952	29·931	29·30	74	53	69	56	60	W.	NW.	calm	...	...	·13
4	30·069	30·060	29·995	29·972	29·45	72	58	69	55	60	SW.	SW.	calm	·36	·210	·27
5	29·985	29·884	29·955	29·922	29·23	69	51	68	58	63	SW.	NW.	W.	·15	...	·35
6	29·926	29·897	29·898	29 892	29·27	76	56	67	54	63·5	SW.	SW.	W.	·01	·085	...
7	30·000	29·901	29·948	29·942	29·32	75	47	68	54	64	SW.	W.	W.	...	...	...
8	30·073	30·062	29·948	29·936	29·47	80	50	68	54	68	SW.	W.	S.	...	...	...
9	30·074	30·057	29·986	29·945	29·47	82	54	71	55	68	E.	SE.	S.	...	·100	...
10	30·223	30·125	30·136	29·992	29·44	85	61	71	62	72	S.	NW.	S.	...	...	...
○ 11	30·313	30·291	30·192	30·186	29·60	81	47	70	55	67	S.	SW.	W.	...	...	...
12	30·301	30·130	30·195	30·046	29·63	81	54	69	56	62	S.	W.	calm	...	...	...
13	29·985	29·892	29·945	29·922	29·40	71	47	68	56	59	NW.	SW.	calm	...	...	...
14	29·890	29·880	29·895	29·832	29·30	75	56	70	58	59·5	E.	N.	calm	...	...	...
15	29·894	29·878	29·792	29·792	29·26	75	52	70	56	63	SW.	SW.	calm	...	·040	·20
16	30·090	29·953	30·142	29·942	29·27	78	48	68	58	66	W.	SW.	W.	...	...	·13
17	30·115	30·065	30·175	30·092	29·45	78	52	70	58	65	W.	W.	W.	·01	·245	...
18	29·936	29·862	29·842	29·842	29·25	68	58	70	54	65	SW.	SW.	W.	·10	·230	...
☾ 19	29·936	29·748	29·892	29·748	29·04	73	46	68	58	64	W.	SW.	W.	·06	...	·27
20	30·158	29·934	29·948	29·942	29·42	77	60	69	54	62·5	W.	SW.	W.	·06	...	...
21	29·910	29·721	29·898	29·698	29·29	72	55	68	59	62	NW.	SE.	NW.	·50	2·040	·02
22	29·834	29·702	29 654	29·648	29·10	73	53	66	53	62	W.	SE.	W.	·01	...	·28
23	29·947	29·903	29·848	29·654	29·30	71	46	66	54	61	W.	W.	W.	·04	...	·04
24	30·057	30·039	29·954	29·798	29·45	74	50	67	53	58	W.	SW.	NW.	·16	...	·04
● 25	29·834	29·754	29·654	29·648	29·30	69	48	61	48	63	S.	SW.	SW.	·30	1·005	...
26	29·846	29·680	29·854	29·757	29·17	67	42	58	50	53	W.	W.	NW.	·06	0·030	·12
27	29·770	29·442	29·754	29·254	29·20	64	54	61	54	50	SW	W.	N.	·34	1·410	·42
28	29·319	29·156	29·404	29·260	28·61	68	49	61	51	61	E.	NW.	S.	·03	0·085	·66
29	29·459	29·330	29·704	29·610	28·64	57	52	62	51	58	W.	NW.	NW.	·29	...	·32
30	29·594	29·504	29·754	29·710	28·85	64	45	61	54	58	W.	NW.	NW.	·26	...	...
31	29·746	29·658	29·660	29·604	29·16	69	53	63	58	56·5	W.	SW.	W.	·16	·160	·39
	30·313	29·156	30·195	29·254	29·27	85	42	71	48	61·8				3·62	5·815	3·89

Remarks.

London.—Aug. 1. Fine: rain. 2. Rain: very sultry: thunder-storm in the afternoon. 3, 4. Cloudy: fine. 5. Rain. 6. Heavy dew: rain. 7, 8. Fine. 9—12. Very hot. 13. Cloudy: fine. 14. Foggy: fine. 15—17. Fine. 18. Showers. 19. Rain: clear. 20. Fine: rain at night. 21. Cloudy: rain. 22. Fine: rain: clear at night. 23. Heavy shower at noon. 24. Fine, with showers. 25. Rain: fine. 26. Overcast. 27. Cloudy: stormy, with heavy rain at night. 28. Rain: fine: overcast at night. 29. Cold rain. 30. Rain: clear. 31. Fine.

Penzance. — August 1. Fair: showers. 2. Fair. 3. Fair: clear. 4. Fair: evening rain. 5. Fair. 6. Fair: showers. 7—10. Fair: clear. 11—14. Fair. 15. Fair: showers. 16. Fair. 17, 18. Fair: 19, 20. Fair. 21. Heavy rain. 22. Fair: showers. 23. Fair. 24. Clear: fair. 25. Heavy showers day and night. 26. Fair: showers. 27. Heavy rain. 28. Fair: showers. 29, 30. Fair. 31. Fair: showers.

Boston.—August 1. Cloudy: rain P.M. 2, 3. Cloudy: rain early A.M. 4. Cloudy: rain P.M. 5. Cloudy: rain early A.M. 6. Fine. 7. Cloudy. 8, 9. Fine. 10. Fine: therm. 81, 4 P.M. 11. Fine. 12—14. Cloudy. 15. Cloudy: rain early A.M.: rain P.M. 16. Cloudy. 17. Fine. 18. Fine. 19. Cloudy. 20, 21. Fine. 22. Cloudy: rain, with thunder P.M. 23. Fine: rain P.M. 24, 25. Fine. 26. Fog: rain, with thunder and lightning P.M. 27. Rain. 28. Cloudy: rain P.M. 29. Cloudy. 30. Cloudy: rain A.M. & P.M. 31. Cloudy.

THE

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[THIRD SERIES.]

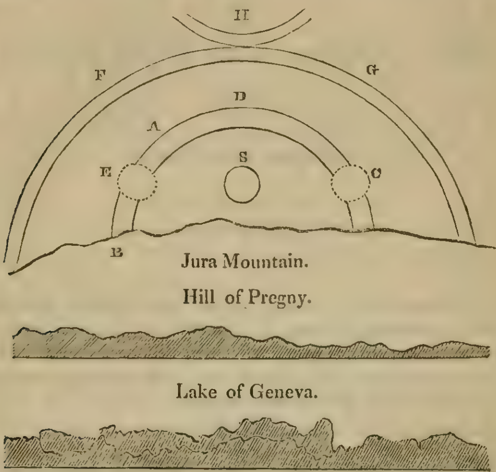
NOVEMBER 1832.

LXI. *Observations on some remarkable Optical Phænomena seen in Switzerland; and on an Optical Phænomenon which occurs on viewing a Figure of a Crystal or geometrical Solid. By* L. A. Necker, *Esq. Professor of Mineralogy at Geneva. In a Letter to* Sir David Brewster.

My Dear Sir,

I MUST not delay any further fulfilling my promise of writing to you, respecting the subjects of our conversation in the too short moments I had the very great pleasure and good fortune of seeing you in London. I am the more bound to write soon, as my first object must be to correct a wrong statement in the date of my observation of the parhelia, as stated in my letter to Mr. Forbes (Edinb. Journal of Science, No. 12. p. 251.). As this mistake, into which I was led by trusting too much to my memory, and to some wrong inferences, has had the effect of weakening some circumstances which were in favour of your explanation, I am the more anxious to do it all justice, so that you may be in time to announce in your next Number the consequences of this mistake, for which I beg leave to apologize to you, to Mr. Forbes, and to your readers, whom I have unwillingly led into error. The true date of the day when I saw the parhelia, was the 1st of June 1830, as I observe by the little memorandum I kept of this remarkable, and to me entirely new phænomenon,—and not the middle of July, as I stated in my letter, written from memory in Edinburgh. Here I transcribe the whole note, together with the little sketch which it contains.

Parhelia seen the 1st of June 1830, from $5\frac{1}{2}$ o'clock P.M. till sunset at $7\frac{1}{4}$ o'clock, represented in the most complete state, as I saw it at 6 o'clock:—S is the sun; EADC the inner halo, which was much the brightest; C and E the two luminous images or mock suns; FG the outer halo, which was



weak, and seen only for a few moments, as well as the inverted arch H. All the various points of these arches were not equally distinct at the same moment as they are represented here. On the contrary, when the image or mock sun at the right hand, C, was strong, the one at the left hand, E, was pale, or did not appear; such was the case at the beginning of the apparition. At the end, the left image, E, was very luminous and coloured, shining with prismatic colours; while the right image C was less visible, and sometimes altogether wanting. The strength of illumination of the various parts of the halo was constantly variable. Often certain portions, sometimes very considerable, entirely disappeared, and afterwards reappeared again. A little before the sun had set, the only part visible was that between A and B, and it was vividly lighted and coloured, and reflected by the lake; at the same time the single point C was also shining brilliantly, coloured with iridescent colours. At the moment of sunset, there remained nothing but a small arch in D. The phænomenon ended almost immediately after the sun had disappeared behind the Jura at 7^{h} 20^{m}. All this time the parts

of the sky situated to the west and north-west were hazy, and with some little clouds; while the eastern and southern parts were perfectly pure and clear, and the chain of the Alps quite pure and bright.

The very rough and inaccurate sketch is a copy of the one which I made rapidly at the time, to preserve the memory of the fact. I well know that the halos and arches must be portions of perfect circles, and parallel to each other; but it being easy to make this correction in the mind, I preferred giving the thing as I sketched it in haste two years ago.

I am happy now to be able to give accurate information about the state of the atmosphere in the day itself of the phænomenon, and in the days preceding it, by referring to the meteorological tables of the *Bibliotheque Universelle*, to which you may look for more particular details. I see that on the 24th of May 1830, the thermometer of Reaumur had stood between 10° minimum and 20°·8 maximum; then came rain; and by my notes I see that snow fell on the 25th on the Jura, which was melted on the 26th. On the St. Bernard (1278 toises above the level of the sea), the 24th of May, the temperature was between +2° R. minimum, and +8° R. maximum, when rain fell, and the thermometer on the 25th of May descended to −0°·2 R. minimum, and +4°·5 R. maximum. On the 27th and 28th of May, snow fell on the St. Bernard, and the temperature decreased till the 29th of May, when it was so low as to reach −6°·1 R. minimum, and +4°·5 maximum. On the 30th of May it had risen again to −4°·8 R. minimum, and +5°·4 maximum; and on the 31st to −1°·1 minimum, and +5°·7 maximum: so much for the temperature of the high parts of the atmosphere at the St. Bernard. During the same time, in the lowland at Geneva, since the rain of the 25th of May, the thermometer had gradually lowered till the 30th of May, when it had attained +3°·2 R. minimum, and +15°·4 R. maximum. On the 31st of May it had already risen to +10° R. minimum, and +14°·4 R. maximum.

Now on the 1st of June 1830, the day of the parhelia, the thermometer at Geneva was between +3°·5 R. minimum, and +17°·3 R. maximum. The last must have been nearly the temperature during the phænomenon in the plain. At the St. Bernard on the same day, the thermometer was between −3°·6 R. minimum and +9° R. maximum. This last temperature may give an idea of that of the atmospheric strata at 1000 toises above Geneva, at the time of the parhelia. The whole day was serene and cloudless at the St. Bernard. At Geneva it was likewise so, except in the afternoon, when a thin mist or haze, and some light clouds, appeared in the west.

From the combination of all these circumstances, it remains not unlikely, nay, even probable, that icy particles may have been floating in those light mists which gave rise to the parhelia, if we suppose their height to exceed a good deal that of 1000 toises above Geneva, or 1280 toises above the level of the sea.

Although mistaken in my former statement of the epoch on which the parhelia took place, considering that June, though not the hottest, as I said of July, is at least a hot month of our summer, that the occurrence of a parhelion in that season, and in such a latitude as ours (46° 12′ N. lat.), is a very rare thing, and that by the knowledge we have been able to get of the meteorological circumstances attending such a phænomenon (circumstances which I do not believe have been mentioned in similar accounts of parhelia),—we are able to form an idea at least of the minimum of height at which the refracting medium causing the parhelia must have been placed. I do not regret to have drawn your attention to this fact, which, instead of militating against, will rather tend to corroborate your ideas as to the necessity of supposing minute crystals of ice to explain the phænomenon.

I now come to the point which you particularly wished me to describe to you: I mean the luminous appearance of trees, shrubs and birds when seen from the foot of a mountain, a little before sun-rise. The wish I had to see again the phænomenon before attempting to describe it, made me detain this letter, a few days, till I had a fine day to go to see it at the Mont Saleve; so yesterday I went there and studied the fact, in elucidation of which I made a little drawing, of which I give you here a copy: it will, with the explanation and the annexed diagram, impart to you, I hope, a correct idea of the phænomenon. You must conceive the observer placed at the foot of a hill interposed between him and the place where the sun is rising, and thus entirely in the shade; the upper margin of the mountain is covered with woods, or detached trees and shrubs, which are projected as dark objects upon a very bright and clear sky, except at the very place where the sun is just going to rise; for there all the trees and shrubs bordering the margin are entirely, branches, leaves, stem, and all, of a pure and brilliant white, appearing extremely bright and luminous, although projected on a most brilliant and luminous sky, as that part of it which surrounds the sun always is. All the minutest details, leaves, twigs, &c. are most delicately preserved, and you would fancy you saw these trees and forests made of the purest silver, with all the skill of the most expert workman. The swallows and other birds flying in those parti-

cular spots appear like sparks of the most brilliant white*. Unfortunately all these details, which add so much to the beauty of this splendid phænomenon, cannot be represented in

Fig. 1.

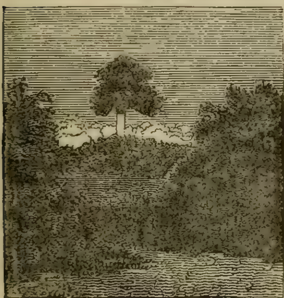


Fig. 2.

such small sketches. Neither the hour of the day, nor the angle which the object makes with the observer, appears to have any effect; for on some occasions I have seen the phænomenon to take place at a very early hour in the morning. Yesterday it was 10 o'clock A.M., when I saw it as represented in fig. 1. I saw it again on the same day at 5 o'clock P.M., at a different place of the same mountain, for which the sun was just setting. At one time the angle of elevation of the lighted white shrubs above the horizon of the spectator was about 20°; while at another place it was only 15°. But the extent of the field illuminated is variable, according to the distance at which the spectator is placed from it. When the object behind which the sun is going to rise, or has just been setting, is very near, no such effect takes place. In the case represented, fig. 1, the distance was about 194 metres, or 636 English feet, from the spectator, in a direct line; the height above his level being 60 metres, or 197 English feet, and the horizontal line drawn from him to the horizontal projection of these points on the plane of his horizon being 160 metres, or 525 English feet, as will be seen in the following diagram, fig. 3. In this case only small

[* This appearance seems to be connected with that assumed by flying birds when seen, under certain circumstances, through a telescope, during observations on the sun, and which, it has been alleged, has occasionally been mistaken for that of small meteors seen in the day-time: see the next two pages.—EDIT.]

shrubs, and the lower half of the stem of a tree, are illuminated white, and the horizontal extent of this effect is also comparatively small; while at other places when I was nearer the edge behind which the sun was going to rise, no such effect took place. But, on the contrary, when I have wit-

Fig. 3.

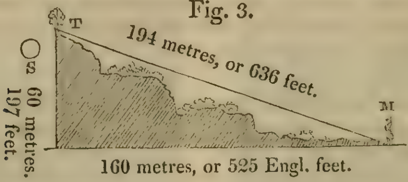
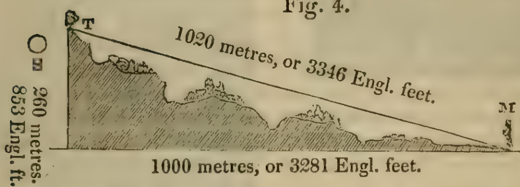


Fig. 4.



S. Apparent place of the sun. T. Tree illuminated white.
M. Spectator.

nessed the phænomenon at a greater distance and at a greater height, as I have seen it other times in the same and in other mountains of the Alps, large tracts of forests and immense spruce firs were illuminated white throughout their whole length, as I have attempted to represent in fig. 2. and the corresponding diagram, fig. 4. Nothing can be finer than these silver-looking spruce forests. At the same time, though at a distance of more than a thousand metres, a vast number of large swallows or swifts (*Cypselus alpinus*), who inhabit those high rocks, were seen in the shape of small brilliant stars or sparks moving rapidly in the air. From these facts, it appears to me obvious that the extent of the illuminated spots varies in a direct ratio of their distance; but at the same time that there must be a constant angular space, corresponding, probably, to the zone, a few minutes of a degree wide, around the sun's disk, which is a limit to the occurrence of the appearance: this would explain how the real extent which it occupies on the earth's surface varies with the relative distance of the spot from the eye of the observer, and accounts also for the phænomenon being never seen in the low country, where I have often looked for it in vain. Now that you are acquainted with the circumstances of the fact, I have no doubt that you will easily observe it in some part or other of your Scotch hills: it may

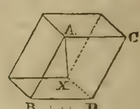
be, some long heaths or furze will play the part of our alpine forests; and I would advise you to try to place a bee-hive in the required position, and it would perfectly represent our swallows, sparks or stars.

I now only wonder that such a phænomenon, which must necessarily take place in every mountain of the earth, every day and at every hour of the day when the sun shines, should never have been noticed before.

I now come to another subject which you also desired me to mention: I mean the varying colours exhibited by Mont Blanc during and after sunset. Lord Minto was perfectly right in the account he gave you of these appearances. But he may have omitted some circumstances which will assist in leading us to an explanation of these varying appearances. I shall here state the facts in the order in which they appear. When the sun is near setting, and the weather is serene, all the mountains of the Alps, facing the west, are tinged with a fine purplish hue, which on Mont Blanc, on account of its bright covering of snow, takes a tinge more verging towards a light orange. When the sun has set for the plain, these mountains appear more vivid and more illuminated, by the effect of contrast. When, some minutes after, the lower mountains are in the shade, their purple hue is changed into a dark blueish tinge, the contrast between their shaded parts and those that were lighted by the sun has disappeared, and an almost uniform grayish blue shade covers them all; at this time Mont Blanc remains the only terrestrial object still lighted by the rays of the sun, and that circumstance causes its immense mass of snow to appear more bright, and its yellowish orange colour more vivid: at the same time the contrast between the projected and other shadows and the lighted parts is at its maximum (I have two or three times seen Mont Blanc at that moment, and when dark clouds were behind it, look almost as bright and red as a live coal). When, however, the sun has set for Mont Blanc, which happens about a quarter of an hour after it has set for the plain round Geneva, then the whole of Mont Blanc assumes a dull blueish white hue, and a flattened appearance, owing to the absence of contrast from the once shaded parts with those that were lighted. And so its new aspect is to that which it offered a few minutes before, like that of a dead body to a living and healthy one. This pale and, as it were, morbid appearance of the mountain is owing to the fact, that above it exists still a wide zone of atmosphere loaded with thin and light vapours, for which the sun has not yet set, and which on that account are still lighted vividly, and coloured with a purple hue. When,

however, the sun has also been setting for these higher regions of the atmosphere, the contrast between their illumination and the shade existing all over Mont Blanc, to which was owing that blueish and deadly colour assumed by the eternal snows, having ceased to take place, the Mont Blanc assumes once more, but in a much fainter and darker manner, its orange yellow colour; and the lower and nearer mountains recover their purplish hue. All the objects then being uniformly and altogether illuminated by the much paler and less powerful light of the twilight, as they were before all lighted at once by the brighter, but equally uniformly spread, light of the sun; so that every thing being placed in the same relative quantity and quality of illumination as before, an analogous aspect is seen in both cases, though much darker under the latter than under the former circumstances. Hence it appears to me that the whole of the phænomenon is most naturally and easily explained by contrast.

The object I have now to call your attention to, is an observation which is also of an optical nature, and which has often occurred to me while examining figures and engraved plates of crystalline forms: I mean a sudden and involuntary change in the apparent position of a crystal or solid represented in an engraved figure. What I mean will be more easily understood from the figure annexed. The rhomboid AX is drawn so that the solid angle A should be seen the nearest to the spectator, and the solid angle X the furthest from him, and that the face ACBD should be the foremost, while the face XDC is behind. But in looking repeatedly at the same figure, you will perceive that at times the apparent position of the rhomboid is so changed that the solid angle X will appear the nearest, and the solid angle A the furthest; and that the face ACDB will recede behind the face XDC, which will come forward; which effect gives to the whole solid a quite contrary apparent inclination. I have been a long time at a loss to understand the reason of the apparently accidental and



involuntary change which I always witnessed in all sorts of forms in books of crystallography. The only thing I could observe was, that at the time the change took place, a particular sensation was felt in the eye (for it takes place as well when seen with only one eye, as with both eyes), which proved to me that it was an optical, and not merely as I had at first thought a mental, operation which was performed. After, however, a more attentive analysis of the fact, it occurred to me, that it was owing to an involuntary change in the adjustment

of the eye for obtaining distinct vision. And that whenever the point of distinct vision on the retina was directed on the angle A, for instance, this angle seen more distinctly than the others was naturally supposed to be nearer and foremost; while the other angles seen indistinctly were supposed to be further, and behind. The reverse took place when the point of distinct vision was brought to bear upon the angle X. This solution being found, I proved that it was the real one by three different ways.

1st, By being able at my will to see the solid in which position I chose, and to make this position vary at pleasure, in looking alternately, with fixed attention, either to the angle A, or to the angle X.

2ndly, While looking steadfastly to the angle A, and seeing the rhomboid in its proper position with the angle A foremost, if without moving either the eye or the figure, I made a convex lens (such as is used in spectacles for long-sightedness,) pass gently from below upwards between the eye and the figure, at the instant when the figure was visible through the glass, the change had taken place, and the solid had assumed the apparent position in which the angle X was the foremost, and that only because, owing to the refraction through the glass, the image of the angle X had come to take the place of the real angle A, and so the point of distinct vision, without being at all moved, had by this means come to bear on the angle X, or rather on its image.

3rdly, If through a hole made with a pin in a card you look at the figure in such a manner that either the angle A or the angle X be hidden, the visible angle will determine the apparent position of the solid, so that this angle will always appear the nearest; it will be impossible to see it in any other way, and consequently there will be no change.

What I have said of the solid angles is equally true of the edges,—those edges upon which the axis of the eye or the central hole of the retina are directed will always appear forward; so that now it appears to me certain that this little, at first so puzzling, phænomenon, depends upon the law of distinct vision.

You surely will draw from all the above communications, many consequences which my ignorance of the subject prevents me from anticipating. You may do what you think most proper with all these observations.

I remain, my dear Sir, with the kindest regard,

Ever most sincerely yours,

Geneva, May 24, 1832. L. A. Necker.

LXII. *Some Facts which appear to be at Variance with the Igneous Hypothesis of Geologists.* *By* Robert W. Fox*.

THE speculations respecting the origin of rocks, and the confidence with which their existing arrangements are sometimes attributed to the agency of fire, induced me to endeavour to ascertain their expansion when heated; and the following are the results of the few experiments I have made.

Pieces of granite increased in bulk when raised to a dull red heat between $\frac{1}{50}$th and $\frac{1}{60}$th part; and contracted on cooling to their original dimensions. I did not detect any difference in these respects whether the granite was measured in the direction of its cleavage, or at right angles to it. At a full red heat decomposition commenced, and vitrification at a white heat.

Porphyritic felspar, from an "*elvan course*," heated to redness, expanded $\frac{1}{52}$ to $\frac{1}{56}$ of its original dimensions; to which it again contracted on cooling.

Different specimens of clay-slate were augmented in size, in the direction of their cleavage, $\frac{1}{65}$ to $\frac{1}{77}$, by a heat scarcely visible in the dark in some instances, and by a full red in others; and when cooled, some of them were found to be permanently enlarged to nearly one half of the extent of the expansion the heat had produced. I could not clearly ascertain the expansion of slate at right angles to its cleavage, from its liability to split, but I think it was less considerable.

Greenstone, at a red heat just visible, increased $\frac{1}{80}$ or thereabouts, and contracted back to nearly its previous bulk when cooled. Serpentine, however, underwent no expansion in any direction that I could appreciate, even when the heat was raised to a full red.

If, then, any of the rocks which are expanded so much by great heat, had their origin from irruptions of matter in igneous fusion, ought they not to abound with fissures in *every direction*, or at least to afford evidence of their having once existed in them, *independently of other contiguous rocks?* This consequence seems to follow from their different expanding and contracting properties, even without adopting the hypothesis of various epochs of formation. Such evidences, however, do not exist in Cornwall at least; but, on the contrary, our mineral veins, as is well known, traverse all the rocks without any necessary change in their size or direction. There are, it is true, frequent instances of the thickness, and other characters of veins being altered in passing from rocks of any given denomination into those of another; but if in some cases they become enlarged, in others it is the reverse, so that no rule can be laid down in this respect. Besides, there is far too great a conformity in the

* Communicated by the Author.

direction of the veins containing similar substances, in any given district, to admit of their being referred to the contraction of the rocks which inclose them. The large "*elvan courses*," or porphyritic dykes, which abound in Cornwall, are even more remarkable than the veins for a considerable degree of parallelism; and their inclination from the perpendicular in descending is greater and more uniform, it being mostly towards the N.W.

Mineral veins having, however, the closest affinity to each other, as it respects their contents and horizontal bearing, are very commonly found to separate widely in descending at angles of 30° or 40°, and upwards; whereas other veins which cross them at large angles at the surface, and consequently in their descent also, for the most part differ entirely in their contents. These well-ascertained points are scarcely less opposed to the hypothesis of veins having originated from fissures resulting from the shrinking of the rocks, which would involve their contemporaneous formation in the same rock, than the indisputable fact I have heretofore referred to,—that the contents of veins change with the rocks they traverse. It may, moreover, be well to mention that veins commonly possess the same general appearances in valleys as in the contiguous hills; and not only do they not exhibit symptoms of having overflowed, but in both situations the metalliferous veins are in general equally furnished with "*gossan*," or other foreign matter overlying the ore*.

It has been urged, that mineral substances do not suffer decomposition or vitrification by heat when under great pressure. It is not necessary to inquire whether there be sufficient proof to establish the correctness of this conclusion, because it can hardly be asserted that such great pressure could have existed at or near the surface, or in the fissures resulting from the contraction of the rocks.

Open fissures, or cavities, are frequently found in some metalliferous veins, and very rarely in others of equal or greater thickness.

The yellow sulphuret of copper, crystallized oxide of tin, and other metallic, as well as earthy combinations, which are found in these cavities, and are easily affected by heat, give no indications of its having ever existed even in a slight degree.

Other facts and arguments might be adduced. But are not those I have alluded to sufficiently decided to show that the hypothesis of the igneous origin of rocks cannot be maintained

* How does this fact accord with the assumed denudation of the valleys? [Is not this query too general? Surely it is *demonstrable* that some valleys have been formed by the process of denudation, however true it may be that that mode of formation has been ascribed to others by assumption merely.—Edit.]

without creating greater difficulties than it tends to explain? If so, surely it ought to be discarded from the science of geology, although no other hypothesis may be substituted for it.

Geologists have sometimes carried their speculations so far as to refer the spheroidal form of the earth to its having once been a mass of plastic matter in igneous fusion or aqueous solution, its present shape being due simply to the operation of mechanical principles. But these appear rather to militate against the assumption, because the rocks, instead of being parallel to the equator, have their prevailing stratification at considerable angles to it in various parts of the world: moreover, the proportion of land to the water between the tropics exceeds that which is near the poles, and the specific gravity of the rocks is equally great; whereas, on mere mechanical principles, the most fluid and lightest matter ought to accumulate near the equator, and the heaviest near the poles.

If their arguments be founded on the adaptation of the earth's form to the rate of its daily revolution,—in which of the works of the great Creator is there not the most perfect and wonderful adaptation to the end designed? And if it cannot be denied that in the beginning it existed in things the most minute, I see no ground for imagining that this great globe, with which the existence of animal and vegetable life is so indispensably connected, should present a solitary exception.

Many of the operations in nature, and the laws which regulate them, may, to a certain extent, be comprehended by man; and the more they become developed, the more beautiful and harmonious they appear: but we cannot find laws to apply to the *original* organization of the earth, or the things which it contains.

The distinction is important in every point of view; and it is surely more useful and instructive to accumulate facts and observations upon the actual state of things and their mutual relations, and to deduce from them such conclusions as experience and analogy may justify, than to hazard conjectures, and puzzle ourselves about questions which probably are, and ever will be, out of our reach.

LXIII. *Description of a Repeating Circle, by which any Multiple of an Altitude may be measured from one Observation by the Telescope.* By JOHN NIXON, *Esq.**

SEVERAL years ago the late Mr. James Allan constructed for me a repeating circle, designed originally for the measurement of (oblique) terrestrial angles, which, when mounted

* Communicated by the Author.

with an additional level, and fixed in a vertical position, would serve not only to take an altitude, but also to obtain, after the completion of one single observation, any multiple of the angle. With the aid of the front view of the circle, fig. 1., and the horizontal section through its axis (fig. 2.), its construction, and the method of using it, may be briefly described.

Fig. 1.

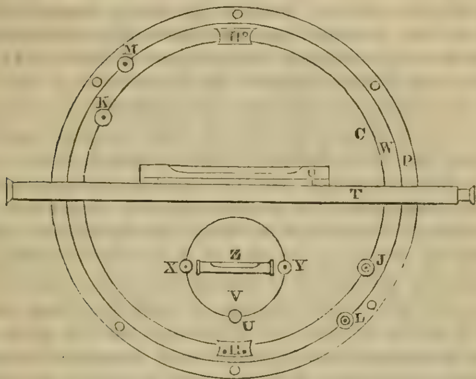
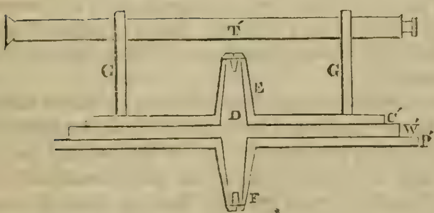


Fig. 2.



W is an eleven-inch wheel, furnished with an axis D, which projects about three inches beyond each surface of the wheel C, a ten-inch graduated circle, of which the hollow axis E moves about the front part of the axis of the wheel. H, H are two opposite verniers, fixed on the wheel, by which the divisions of the circle are read off to $10''$. P is an immoveable (circular) plate, twelve inches in diameter, having a hollow axis F, in which the back part of the axis of the wheel is fitted and revolves. The two-foot achromatic telescope T, carrying a spirit-level U, rests with its cylindrical rings within two Ys,

G, G, fixed to the divided circle. When the instrument is placed in a horizontal position, the line of collimation of the telescope can be rendered level (in the usual manner) by the adjustments of the level and those of the cross-wires. It will also be parallel to the plane of contact of the wheel to the circle in case the latter can be moved half-round in azimuth without disturbing the bubble (of the telescope). The circle C and the wheel W have their circumferences cut into teeth. One pinion J (fixed to the wheel) serves to move the circle, and the other L (fixed to the immoveable plate) turns the wheel and circle, clamped together by the nut K. The wheel and plate are secured together by the nut M.

Supposing the instrument fixed by the plate P to the side of a vertical wall, or mounted as a French repeating circle, with a movement in azimuth; the circle and wheel, clamped together with the zero lines of the divisions of the circle coincident with those of their respective verniers, are moved together by the pinion L, until the middle of the bubble is brought to its reversing point. The line of collimation of the *adjusted* telescope will now point level; and if we wish to take the altitude of a nearly horizontal star, we must disengage the circle from the wheel, and direct the telescope exactly at the star by turning the pinion J. Having clamped the circle to the wheel, the angle of elevation is finally read off by the two verniers. In the event of the subsequent obscuration of the star, note the position of the bubble, and having freed the wheel from the fixed plate, depress the telescope by the pinion L until the bubble attains the reversing point, or that degree of its scale at which the line of sight points level. Clamp once more the wheel to the plate, and having disengaged the circle from the wheel, elevate the telescope (and with it the divided circle) by means of the pinion J, until the bubble reverts to the two points of its scale between which it stood when the telescope bisected the star. The angle now to be read off will, it is evident, be *double* the altitude of the star; and by continuing the same process of measurement, a multiple of the angle, sufficient to obviate the errors of graduation and reading off, may be procured *leisurely* and accurately.

When the altitude of the object is beyond the range of the scale of the level, recourse must be had to the additional level Z mounted on a toothed wheel V, which can be moved by the pinion U about a short horizontal axis projecting from the divided circle, or fixed by the opposite clamps X, Y. Having pointed the telescope parallel to the horizon, and afterwards on the elevated star precisely after the manner above indicated, clamp together the circle C and wheel W, and then

bring the bubble of the additional level by means of its pinion exactly to between the two marks drawn across its tube*. Continue thus successively to depress the telescope by the pinion L until its bubble stands at the reversing point, and afterwards to elevate it by the pinion J until the bubble of the additional level comes to between its marks; and when the repetition has been carried far enough, the altitude may be found by dividing the mean of the two readings by the number of observations.

Apparently, there would be no difficulty in obtaining any multiple of the *double* zenith distance of a celestial object procured precisely as by the French circle. *Two* additional levels, both fixed to the divided circle, would then be indispensable; one to be *levelled* when the instrument had been turned half round in azimuth, and the other when the telescope had been pointed the second time at the star.

By means of the two additional levels, the difference of zenith distance of two objects might be measured at once, without obtaining the absolute zenith distance of either.

Leeds, Sept. 4, 1832. J. Nixon.

Erratum.—Page 108, line 1, for 11″ read 11′.

LXIV. *Investigation of certain remarkable and unexplained Phænomena of Vision, in which they are traced to Functional Actions of the Brain. By* Mr. Thomas Smith, *Surgeon, Fochabers.*

[Concluded from p. 258.]

HAVING thus ascertained the precise nature of the effects, the next step I took was to investigate the true nature of the exciting cause. In all the experiments in which I had hitherto observed the phænomena, the light which appeared to excite them was more or less *white*; for the bright object was either a lamp or candle, or the direct rays of the sun, or the sun's light reflected from snow or the like. In addition to this circumstance, the bright object had always been so situated in relation to the point P, to which the eyes were accommodated, that its image on the retina must have been formed either

* It would be preferable to have both levels fitted up with accurate scales, and to note the position of their bubbles rather than to attempt to bring them always to one fixed mark. From the register of the deviations of the bubble of the telescope from its reversing point, and those of the bubble of the additional level from the point at which it stood when the telescope pointed at the star, it can be ascertained how many seconds are to be added to or subtracted from the final reading.

before or behind the true focal point. Now, as in the investigation of new phænomena, with the principles of which we are, as in the present case, utterly unacquainted, every combination of the circumstances ought, as far as possible, to be tried till we arrive at the most simple which is capable of producing the results; so it appeared proper to try if any one kind of light, or any one position of it, was more efficacious than another in exciting the appearances. The following experiment, repeated often with the utmost care, convinced me that an *imperfect* image of the bright object was required to fall on the retina in order to produce the phænomena.

Exp. 8. I placed a strong bright light at the nearest distance to which my vision could adapt itself, and directing both eyes to it, I caused a screen to be interposed between one of them and the light; so that one of my eyes only was exposed to the bright light, and the image of it was formed perfectly distinct on the retina. A slip of *white* paper, illuminated from behind me, was held so near my eyes as to appear double; the result was very remarkable. Of the two images, that which was seen by the exposed eye appeared darker than that which was seen by the shaded eye; but *both appeared distinctly white*, without any tinge whatever of green or red. In performing this experiment, great caution is required that the exposed eye be adapted correctly to the distinct vision of the flame; for by much observation I have found that a small error in this respect, such as occurs when the eye becomes dazzled, is sufficient to excite those changes in the sensibility to red light, which have been proved to be the causes of the green and red appearances of the white paper.

The difference of brightness observed in the two images in this experiment is undoubtedly owing to the operation of the affection of sight, mentioned in Note *, p. 255, &c. as may be proved by shading or exposing both eyes, by turns. When the images appear unequally bright, by shading both eyes, the darker image acquires the same luminousness as the brighter one; and by exposing both, the bright image becomes of the same shade as the dark one.

Having thus ascertained that bright light failed in eliciting the phænomena when it formed a distinct image on the retina, it remained to try the effects of different kinds of light. The results of numerous observations carefully made with the different primary colours, are shown in the following experiment.

Exp. 9. I raised a broad yellow flame in the manner recommended by Sir David Brewster for the construction of a monochromatic lamp: this I placed in the position F, fig. 1, near my right eye, and applied a tube, blackened within, to my

other eye, to prevent the result from being disturbed by any stronger light entering my left eye. A lighted candle was placed in a dark lantern behind me, with only a small opening in it to permit a stream of white light to fall on the slip of white paper S, which was placed so as to be visible to both eyes when they were directed to a distant point P. The experiments, with the other primary colours, were made with narrow tubes of thin coloured paper. One of the tubes being applied to the right eye, was strongly illuminated by means of lights placed near its sides; and a black tube was applied to the left eye, to insure that inequality of the action of the coloured light on the two eyes, which, even in a more moderate degree, had been found sufficient with *white* light to produce the phænomena: the results in all these trials were striking and uniform. The image of S, seen by the eye exposed to the primary coloured light, was constantly of the colour that was complementary to that of the tube or light employed,—an appearance manifestly referrible to the affection of sight mentioned in Note *, p. 255, &c.; but the image of S that was seen through the black tube was uniformly *white*, being never in the smallest perceptible degree changed by the affection of the other eye.

From these observations we learn that no excess of any primary coloured light entering one of the eyes is able to produce the affection which we have been investigating; hence it follows that *white light* only is capable of exciting it. But the 8th experiment proves that *white light also fails* to produce it, when it forms a *distinct image* on the retina. It is not the action of the white light, therefore, but the *indistinctness of the white image*, that constitutes the true *exciting cause.*

This conclusion leads us to remark, that the affection of vision now under investigation, as well as that which is disclosed under Notes * and †, p. 255 and 257, are both produced by the same exciting cause, or at least by causes of the very same nature. Before inquiring, therefore, into the intimate causes or seat of these analogous affections, it may be of use to compare the indistinct images in both with one another, and with their respective effects, in order to detect, if possible, any physical differences between them and their distinct images that may serve to account for the phænomena they produce.

When rays of light from a *primary* coloured object are intercepted by the retina before they reach their focal point, the image is rendered indistinct by the diffusion and mixing of rays from single points of the object over many points of the retina. Rays from a *white* object similarly intercepted, have, in addition to this cause of indistinctness, another, arising from

the chromatic aberration; for every white image falling on the retina before or behind its true focal point is surrounded by a *red* or *violet* border, which, as well as the other cause, interferes with its distinctness. Now it is certainly a very extraordinary circumstance, that diminished sensibility to red light around the white image should occur then, and then only, when it is surrounded by this red or violet border. A distinct red border around a distinct bright object produces no such effects, as I have proved by experiments carefully made: it follows, therefore, incontrovertibly, that it is not the physical difference between a distinct and an indistinct white image that excites those changes in the sensibility which have been proved to occur. In regard to a primary coloured image, the difference between it when distinct and when indistinct, consists in that diffusion and mixing of the rays in the latter which has been noticed above, and which not only obscure the outline, but the whole surface. If these scattered rays, therefore, produce the changes in the sensibility that take place in these circumstances, we must be compelled to acknowledge that the same physical cause produces directly contrary effects at the same time; for, if this be true, the scattered rays that obscure the surface of the primary coloured image, increase the sensibility, in a remarkable degree, to the same kind of light, and the scattered rays that obscure the outline, diminish, in the same degree, the sensibility to the same kind of light. The supposition is manifestly absurd, and therefore we return with increased confidence to our first conclusions, that *indistinctness of a white image* is the true exciting cause of the *diminished sensibility to red light* that takes place around it in the exposed eye, and of the *increased sensibility to red light* that occurs in the other eye at the same time; and that *indistinctness of a primary coloured image* is the real exciting cause of the *increased sensibility to that colour* which ensues to the image itself, and the *diminished sensibility to the same colour* that occurs for some considerable space around the image.

The nature of the effects and the true exciting causes of these remarkable affections of sight being ascertained, it only remains to investigate the *seat and nature of the actions* excited by the indistinctness of the image on the retina.

In the first place, then, the *retina*, though it has been customary to consider it as the seat of any changes in the sensibility to light, cannot, in these cases, be regarded as the seat of either of these affections in the *exposed eye;* for it is inconceivable that undulatory motions, extending from the part of the retina on which the bright light falls, to all parts around it, can be produced by an *indistinct* image, when a *distinct*

image of equal or superior brilliancy produces no such effect: besides, it has been shown above, that to ascribe the state of the sensibility in and around the part of the retina where the bright light falls, to the physical impulse of the difference between a distinct and an indistinct image, would be to assert a physical absurdity; it follows unavoidably, therefore, that the changes in the sensibility in the exposed eye arise from actions *ab interno.* With regard to the state of the sensibility in the *unexposed* eye in one of these affections, there cannot be two opinions: it must be produced by an action from within; and though we are not yet prepared to say in what manner, or by what medium, an action of the brain can affect the sensibility to light, yet the fact is a most important one; and the perfectly correct manner in which the defect of sensibility in the exposed eye is balanced by an excess of sensibility in the unexposed eye, not only affords an additional argument for their common origin, but seems to open a path, which, if duly followed, may lead to interesting discoveries in this obscure department of physiology. In the other affection of sight, though the changes in the sensibility are confined to the exposed eye only, yet the same correct balance of excess and defect is found to exist,—a circumstance that strongly corroborates the conclusion, that both of these remarkable affections of vision are produced by one common principle, and arise from one common seat. But, in the second place, the *exciting causes* of these affections are of such a nature, I should humbly submit, as to render them totally incapable of producing any but *functional actions* pre-organized for the occasion. *Indistinctness of image* (disregarding its physical causes, which have been shown to be inadequate to the effects,) is a purely negative quality, and can have no effects beyond the simple perception of it, except in so far as distinct vision is the end to which the mechanism of its several organs, viz. the eye, optic nerve, and brain, has been adapted: that of these three the brain is the *directing organ* in another highly interesting function of vision, which is also excited into action by *indistinctness of image*, admits of another kind of proof: the function I mean is that by which the eyes are accommodated to the different distances of objects. Suppose, after looking at a distant object, that we wish to view one near at hand, what we do in this case is to direct the two eyes so as to make their axes meet in the object to be viewed. If the former accommodation of the eyes continued, the object now viewed would be *indistinct;* but to prevent this, an action of the brain produces a change in the eye not yet sufficiently understood, by which a correct image of the object is formed on the retina. That the unknown change here mentioned is pro-

duced by a function of the brain will not, I believe, be disputed, since injury of the brain by mechanical compression, &c. destroys the function, and the contractions and dilatations of the iris that are observed to accompany its exercise. In this case, therefore, distinct vision is the end for which a certain function of the brain, as well as a certain mechanism of the eye, is provided; and the following considerations, suggested by the effects on the objects produced by the affections we are investigating, lead to similar conclusions in regard to the end or purpose of them. It is well known that the vision cannot be adapted in the common way to more than one distance at once. Now when the eyes are adapted in this way to objects at one distance, objects that are nearer or further off than that distance, are actually made more distinct than they would otherwise be by the operation of the two affections we have been examining. By one of these the indistinctness of a brighter object is lessened by the sensibility being increased to the colour of the object itself, and diminished to the same colour in less luminous objects around it, thus making the principal object brighter and better defined by a double contrast. By the other, the indistinctness arising from the chromatic aberration is removed by insensibility to the red or violet rays bordering the image; and as that insensibility extends over a wider space than the red or violet border occupies, the false vision thus occasioned is corrected by the sensibility to red light in the other eye being increased, in exactly the same degree as it is diminished in the exposed eye. Both affections, therefore, have the characters of perfect functions admirably contrived, it must be acknowledged, and as well adapted to produce distinct vision as can well be imagined in the circumstances.

Having thus given, in as compressed a form as I could adopt in justice to the subject, a full account of this investigation, I forbear, for the present, from making any observations on the singular nature of the *cerebral functions* thus detected, or on the perhaps still more singular nature of their *exciting causes*, thinking it due to truth, in a case that appears to involve principles entirely new, to wait the observations of competent inquirers, with whom it remains to confirm or refute, by an impartial scrutiny, the results which I have obtained. I shall, therefore, conclude with a short summary of those results.

1st. Besides the well-known function by which the eyes are adapted, by turns, to different distances, *two other functions*, hitherto unknown, are occasionally called to the aid of vision.

2nd. Both of these newly observed functions are excited by

indistinctness of vision; the one, when the indistinctness arises from undue scattering of the rays of light,—the other, when it is owing to the chromatic aberration of white light.

3rd. The organ excited in both cases is the *brain;* but whether, being thus excited, it does not also excite some other auxiliary organ, as in the case of the adaptation to different distances, does not yet appear.

4th. The actions excited are directed to the effect of removing, more or less, the exciting cause, and producing distinct vision.

Fochabers, 20th June, 1832.

Note.—An investigation of the remarkable phænomena described in the preceding ingenious paper, but leading to results different from those obtained by the author, will be published in the next Number of this Journal.—D. B.

LXV. *On the Lower or Ganister Coal Series of Yorkshire.* *By* JOHN PHILLIPS, *F.G.S., Sec. Y. P. S., Assist. Sec. Brit. Association, &c.**

THE lowest portion of the Yorkshire coal strata resting upon the millstone grit, produces comparatively but a small quantity of coal, and this, in general, not of a good quality. But no part of the coal-field is more curious in its geological relations, or more worthy of close study by those who desire to penetrate into the history of the production of coal. We may define this lowest coal series very simply, by saying that it is included between the millstone grit of Bramley beneath, and the flagstone of Elland above, having a thickness of about 120 or 150 yards, and inclosing near the bottom two thin seams of coal, one or both of them workable, and several other layers scattered through its mass, too thin to be worth working. The most regular and continuous of all these coal seams reaches, in a few places, the thickness of 27 or 30 inches, but is generally only about 16 inches. It is worked at Yeadon, Rawdon, and Horsforth, near Leeds; at Baildon, and Heaton, near Bradford; Catharine Sluck, and Swan Banks, near Halifax; Bullhouses, near Penistone; and at several points about Sheffield.

It would have been impossible to have traced so thin a seam of coal along so extensive a range without some peculiar facilities,—some points of reference more distinct than the varying quality of the coal, and the still more irregular fluctuations of the sandstones and shales. This coal seam is covered by a roof unlike that of any other coal bed, above the mountain

* Read before the Yorkshire Philosophical Society, October 2, 1832; and communicated by the Council of that Society.

limestone, in the British Islands; for, instead of containing only the remains of plants or fresh-water shells, it is filled with a considerable diversity of *marine shells*, belonging to the genera *Pecten* and *Ammonites*; and in one locality, near Halifax, specimens of *Orthocera*, *Ostrea?*, and scaly fish have been obtained from certain nodular argillo-calcareous concretions, called Baum Pots, lying over it. The uniform occurrence of the Pectens and Ammonites through so wide a range over one particular thin bed of coal, while they are not found in any other part of the coal strata, is one of the most curious phænomena yet observed concerning the distribution of organic remains, and will undoubtedly be found of the highest importance in all deductions relating to the circumstances which attended the production of coal. The following sections will convey a good idea of the general character of the whole of this lowest coal series.

The first is a section of Swan Banks Colliery near Halifax; furnished to me by its friendly proprietor, Christopher Rawson, Esq., President of the Literary and Philosophical Society of Halifax, whose remarkable zeal and diligence in exploring the phænomena presented by his underground works, have not only produced the discovery of many new and curious animal remains in the baum pots, but added some very important facts to the general history of the coal-field.

	Yds.	Ft.	In.
Ragstone, (*the lower part of the flagstone rock*) . .	27	0	0
Black shale	40	0	0
Coal (80 yards band coal)	0	0	6
Rag	4	0	0
Black shale	28	0	0
Coal (48 yards band coal)	0	0	11
Gray shale	0	2	0
Black shale	7	1	3
Dirt band (black tough clay)	0	0	3
Black shale	4	1	6
Coal (36 yards coal band)	0	0	7
Galliard	1	0	0
Black shale	12	0	0
Rag and shale	13	0	0
Black shale	7	0	0
Shale and ironstone (called Hard Band), flat baum pots	0	2	3
Gray shale (called White Earth), with small round baum pots containing *Ostreæ?*	1	0	0
Concretions (called Baum Pots), with Ammonites, &c.	0	1	0
Black shale (called Moon Bassett), with Pectens	0	1	0

	Yds.	Ft.	In.
Coal (the hard band coal) worked	0	2	3
Seatstone	0	1	0
Seat earth (white clay), with vegetable fossils	2	0	0
Gray shale	5	0	0
Black shale	4	1	6
Coal (middle band coal)	0	0	10
Middle band stone	1	1	0
Black shale	8	0	0
Layer of fresh-water shells (*Unio*)	0	1	0
Black shale	3	1	0
Ironstone	0	0	3
Black shale	0	0	8
Coal (the soft bed coal) workable	0	1	5
Upper millstone grit, on which Halifax stands.			

The following section of Horsforth Colliery was communicated to me by my most valued and lamented friend the late E. S. George, Esq., of Leeds, whose researches into the minuter history of the Yorkshire coal strata were equally accurate and well directed; and I hope that some of their results will soon be brought before the public.

	Yds.	Ft.	In.
Clay	2	0	0
Galliard of Headingley Moor	0	2	6
Galliard in thin beds	2	1	6
Coal (sometimes thickened by stone coal)	0	0	4
Sandstone with partings	4	0	0
Blue-brown rag	4	0	0
Lifts or beds of sandstone, about 14 inches thick, with 2-inch partings of shale	6	0	0
Blue shale	6	0	0
Sandstone	2	0	0
Blue shale	18	0	0
Blue shale, with *Ammonites Listeri*	0	0	3
Blue shale, with *Pecten papyraceus*	0	1	0
Coal	0	1	4
Seat of coal (White Earth)	2	0	0
Measures of stone and shale, with a seam of ironstone	8	0	0
Coal	0	1	0

Various measures occur below to the thickness of 20 or 30 yards, and then the millstone grit of Bramley appears.

In these sections, we may observe, besides the very remarkable layers of marine shells, several occurrences of a peculiar hard siliceous sandstone, called Galliards, Ganister, or Seatstone (according to local custom, or slight differences),

which in fact is the same thing as the "crowstone" of the mountain limestone district in the north-west of Yorkshire, and like that contains in abundance the remains of plants, particularly of the genus *Stigmaria*, Brongn. By the extreme abundance of plants of this kind, indeed, the galliard beds may almost always be recognised throughout their range in Yorkshire. This stone, in some cases, forms the floor or sill of the coal seams,—a circumstance never observed in the upper coal strata, amongst which, indeed, galliard never occurs in its true character. Hence this whole group of strata may be appropriately called the Galliard or Ganister coal series.

The Ammonites and Pectens which lie above one of the seams of coal, and still more the *Orthoceræ* which sometimes accompany them, are remarkably analogous to fossils of the mountain limestone. The galliard is likewise to be compared with similar stones in the mountain limestone series, and therefore the ganister coal series might without impropriety be associated with the upper-mountain-limestone series of the Penine chain, or with the millstone grit and limestone shale of Derbyshire, and thus the flagstone of Elland would appear to be the lower limit of the true coal-measures. But a short examination of the neighbourhood of Halifax, in October 1831, has shown me another order of phænomena and another set of shells, which connect this same series with the upper or true coal-measures.

In the upper coal series of Northumberland, Durham, Yorkshire, and Derbyshire, are several most extensive layers of bivalve shells, commonly called Muscle bands, and referred to the genus *Unio*, from which the fresh-water origin of those coal deposits has been inferred. It was therefore with extreme gratification that I found, in passing through Mr. Rawson's colliery at Swan Banks, in the midst of this series of ganister coals, two layers of these shells, one of them about the middle of the series, considerably *above the Pecten coal*, the other near the bottom, and considerably *below that coal*.

No shells of this kind have ever been met with in the mountain limestone group, which there is every reason to consider as of decidedly marine origin;—not one of all the zoophytic, testaceous, or crustaceous reliquiæ of this limestone has ever been found in the upper coal series: this opposition of zoological characters would appear to be fully explained, if the coal deposits were admitted to have been accumulated in fresh water, and this opinion is, perhaps, generally adopted.

We find, then, in the lowest coal series, which is placed on the line of transition between the marine and fresh-water de-

posits, zoological and mineralogical characters common to both. Examined in detail, we find these characters not mixed, but alternating in such a manner as if there had been one periodical return of the marine element into its ancient receptacle, after that had been for some time occupied by fresh water and its few inhabitants. The effects of this irruption having, as it were, worn out, the zoological characters of fresh-water deposits are again manifested at intervals in the muscle bands, till the whole carboniferous system is entirely ended, and marine exuviæ reappear in the magnesian limestone.

If, from whatever cause, we could witness the effects of a general irruption of sea-water into a modern lake of great extent and considerable depth, it is probable that the resulting phænomena would be perfectly analogous in kind to those described above. But this irruption of the ancient ocean into the coal-basin of Yorkshire was probably not produced by any violent convulsion *in that basin*,—for there is *no unconformity* between the supposed marine and supposed fresh-water deposits,—but by some disturbing causes originating at a distance. As the elevation of the Western Alps has probably occasioned the dispersion of boulders in Dauphiné and Provence, and as the uplifting of the Scandinavian chain has been followed by diluvial currents in Germany, without affecting the position of the strata in those countries, so may the Yorkshire coal district have felt the transient shock of some distant convulsion.

The periodical revolution in the nature of the waters which operated the deposition of the lower coal strata in Yorkshire, bears so remarkable an analogy to some of the phænomena of the marino-lacustrine tertiary deposits, that the same principles will probably serve for a basis for the explanation of both cases. In both instances we have *a decidedly marine deposit below*; and a *decidedly lacustrine deposit above*; the intermediate ground is not exactly neutral, but sometimes shows *gradations* from the one to the other, and sometimes *periodical alternations*,—accompanied, however, by so entire a parallelism of strata, that in seeking for the cause of these changes, we are compelled to have recourse to *agency at a distance*,—to the blocking up of the outlet of some estuary, or to irruptions of the sea arising from subterranean disturbances in a different quarter.

In a future communication to the Society, I shall describe in detail all the species of animal remains which have been obtained from this interesting part of the Yorkshire coal strata.

LXVI. *Official Documents respecting the Health of the Workmen employed in Cleansing the Public Sewers of Westminster, as affected or not by their Employment, and also during the existence of Malignant Cholera in the Metropolis; together with authenticated Statements relative to the Health of other Workmen exposed to putrid Effluvia. Communicated in a Letter from* Sir Anthony Carlisle, *F.R.S. &c. &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE accompanying documents have been for some time in my possession, but I deemed it proper to reserve them from public notice until the heated contentions about Indian cholera were abated. In the capacity of a Commissioner of Sewers for Westminster, I requested authenticated information respecting the effects of putrid substances on the health of persons employed for the longest time under the orders of the Commissioners; and especially whether they were particularly affected by putrid fevers or by bowel complaints. The tabulated report now submitted for publication is left to be used at my discretion; and the facts appearing in many instances contrary to general opinion, yet unquestionably accurate, I think them well deserving the notice of medical philosophers, and of other scientific men.

A similar report was obtained by Commissioners appointed by the Council of Health at Paris, in 1826, to inquire into the effects of putrescence in certain French manufactures of catgut and other strings made from animal intestines, with a view to ascertain the antiseptic influence of chlorides. The several results, published by order of the French Commissioners, have been translated and published, with many additions, by M. Labarraque and the translator, in 1827, under the title of an Essay on the Use of the Chlorurets of Soda and Lime, &c. &c. by Thomas Alcock, Surgeon. Printed for Burgess and Hill.

Since the report above mentioned was received, I have thought it needful to make inquiries also respecting the effects of the late prevailing malignant cholera among the labourers in the sewers of Westminster; and I now send you the official returns, which appear to me of much public importance.

I cannot imagine that any person will regard the publication of these facts, so impartial and genuine, as an encouragement of filthiness; since they are entirely and specially directed to discover whether any connection subsists between the origin or propagation of malignant cholera, and the most offensive and varied kinds of putrid vapours.

I am, Gentlemen, your obliged Servant,

Langham Place, Oct. 6, 1832. Anthony Carlisle.

Report on the Health of the Workmen employed in Cleansing the Public Sewers of Westminster, &c.: transmitted to SIR A. CARLISLE, *by Mr.* J. HOUSEMAN, *Clerk to the Commissioners of Sewers for Westminster; and dated December* 10, 1831.

Designation of Workmen*.	Age.	No. of Years' employed.	Putrid Fever.	Slight Colds; Pains in the † Bowels, Loins, and Limbs.	Wounded by broken Glass Bottles in the Sewer.	Burnt by Gas.	Burnt by Foul Air.
1. Labourer	60	22	...	Often pains in the bowels without being relaxed; took spirits to relieve him.	Once by a bone in the soil, running into his foot; ill five weeks.		Once while letting off soil from a privy, drain and cess-pool, South Molton Lane; face and whiskers burnt slightly, eye rather injured;—two years ago.
2. Ditto...	52	22	...	Often pains in the bowels and limbs from cold caught standing in sewers.			
3. Ditto...	42	21	...	Often pains in the bowels and limbs from cold caught standing in sewers.	...	...	
4. Ditto...	50	20	...	Often, once very severely, has had an apoplectic fit.	Often.		
5. Ditto; formerly a gentleman's servant.	45	17	...	Often with slight bowel complaints.	Once very severely by a glass bottle; six years ago.		
6. Labourer	35	17	...	Often colds, pains in the limbs and loins	Often.	Once very severely in Great Queen-street, by Drury-lane, November 1831.	
7. Ditto ...	46	17	...	Often slight colds; once with inflammation, ill three weeks	Often.	Was nearly suffocated by an explosion of gas under a man-hole in Northumberland-street; the labourer that was with him was burnt (not now in the works).	
8. Ditto...	42	14	...	Ditto ditto; ill two weeks . .	Once.		
9. Ditto...	54	17					
10. Ditto; 20 years a horse soldier. . .	54	16	...	Once by being washed into the Thames from St. Martin's lane sewer	. . .		

* In the original Report, the names of the several workmen are given in this column; but thinking it unnecessary to publish these, we have substituted numbers for them; and in the Supplement, p. 359, letters.—EDIT.

† Convenience of printing has required the omission, in this page, of the *blank* columns headed "Flux" and "Sick from Stench."—EDIT.

Designation of Workmen.	Age.	No. of Years employed.	Flux.*	Slight Colds; Pains in the Bowels, Loins, and Limbs.	Sick from Stench.	Wounded by broken Glass Bottles in the Sewer.	Burnt by Gas.	Burnt by Foul Air.
11. Labourer.	30	13	...	Often slight colds, with pains in loins and limbs.				
12. Ditto ...	31	10	...	Ditto.	Once in Dartmouth Row, Westminster, five years ago; so foul as to put the candles out.			
13. Bricklayer	46	21	...	...	Often slight colds.	Sometimes.	Once under a man-hole in Praed-street, Paddington; ill two weeks.	
14. Labourer.	48	13	...	Often slight colds.				
15. Ditto ...	50	10	...	...	...	Once in Nottingham Place; ill two weeks.		
16. Ditto ...	45	14	...	Once.				
17. Ditto; 14 years a livery-servant. .	37	14	...	Once very severely with fever, ten years ago, Mary-le-bone-lane.		Once very severely in the Blacklands open sewer; on crutches 9 months.	Once very severely in Mount-street, Grosvenor-square; in St. George's Hospital many weeks.	Once slightly in Clarges-street, May Fair, this month.
18. Labourer.	37	10	...	Once very severe cold and bruises from being washed into the Thames from Essex-street, with 4 others.				
19. Ditto ...	43	14	...	Often.		Often.		
20. Ditto ...	38	6	...	Often.				
21. Ditto ...	32	10	...	...		Once.		
22. English Ditto ...	28	5	Once with swellings in throat; five weeks in Gerard St. Disp.; thinks it was cleansing sewer back of Tothill Fields Bridewell.	Often.	Often; this day in Ryder's court, St. Ann's.	Often.		

* *Blank* column "Putrid Fever" omitted.—Edit.

Designation of Workmen.	Age.	No. of Years employed.	Putrid Fever.	Flux.	Slight Colds; Pains in the Bowels, Loins, and Limbs.	Sick from Stench.	Wounded by broken Glass Bottles in the Sewer.*
23. Ground-man....	51	11	...	...	Often.	Sometimes.	
24. Bricklayer..	39	7	...	Once in Hemlock-court, Carey-street, August last.			
25. Labourer ..	30	10	Caught a bad fever from a bed-fellow, in Crown-street, St. Giles's, 7 years ago.		Often.		
26. Ditto; was a miller ...	29	5	...	...	Often.		
27. Labourer ..	26	7	...	...	Often.	...	Often.
28. Ditto......	24	6	...	...	Often.	...	Often.
29. Ditto; was a distiller's fireman: very subject to ague......	41	11	...	...	Often.		
30. Labourer ..	24	6	...	...	Once.		
31. Ditto......	31	7	...	...	Twice.		
32. Ditto......	25	5	...	...	...	...	Often.
33. Ditto......	35	6	...	...	...	...	Often.
34. Ditto; was a grocer's shopman	33	6	...	...	Often colds, with bowel complaints.		
35. Labourer ..	54	9	...	...	Once very severe cold from being washed into the Thames from Essex-street; ill four months.		Often.
36. Ditto; a very sober man; continually cleansing sewers: never ill the first four years of his employment.	34	9	...	...	Often has very severe bowel complaints, lasting two or three weeks at a time.	Often in Short's Gardens, St Giles.	Often.
37. Labourer ..	30	13	...	...	Severe cold and fever, nine years ago, in Princes-street, St. Ann's; confined in St. Giles's workhouse.		

* *Blank* columns "Burnt by Gas" and "by Foul Air" omitted.—Edit.

Designation of Workmen.	Age.	No. of Years employed.	Putrid Fever.	Slight Colds; Pains in the Bowels, Loins, and Limbs.	Wounded by broken Glass Bottles in the Sewer.	Burnt by Gas.*
38. Labourer . . .	36	10	...	Often.	Often.	
39. Ditto; formerly a parish-clerk and schoolmaster in Ireland .	55	10	...	...	Once, two years ago, confined fifteen weeks in the Middlesex Hospital.	
40. Labourer; formerly a shoe-maker; the business did not agree with him: never ill since working in the sewers ..	33	8				
41. Labourer . . .	30	11	...	Once with pains in the loins.		
42. Ditto	28	5	...	Once.		
43. Ditto; formerly a linen comber, and a smith's labourer;— not very good health	40	12	Once his wife and children caught it of him; one of the children died in St. Giles's workhouse, three years old: he was at work in the Ranelagh Plain, casting out soil, dead dogs, &c.	Often with pains in the bowels.	Often; once very severely in the Ranelagh; ill two weeks.	
44.	48	12	...	...	...	Once, in Compton-street, by Dean-street.
45.	40	10	...	Often colds; once a very bad cold, and very much bruised, having been washed into the Thames from Essex-street, with four others; confined in the Middlesex Hospital.		

* *Blank* columns "Flux," "Sick, &c." and "Burnt by Foul Air," omitted. —EDIT.

Letter addressed to Sir A. Carlisle from Mr. J. Houseman, Clerk to the Commissioners.

Sewers' Office, Greek Street, Soho, 6th Oct. 1832.

Sir,—In compliance with the request in your letter of the 2nd of this month, I have made inquiry of the Surveyor, "whether any, or, if any, how many workmen or others, employed under the Westminster Commission of Sewers, have been attacked with Indian cholera; and if so, what number have died:" and I beg leave to send you herewith the Surveyor's reply. I have the honour to be, Sir,

Your most obedient Servant,

JOHN HOUSEMAN, Clerk.

Sir Anthony Carlisle, &c. &c.

The following is the Report alluded to in the above Letter.

Sewers' Office for Westminster, Oct. 3, 1832.

The average number of men employed in *cleansing* the sewers, under the jurisdiction of this commission, is fifty-four; of whom, only four have been attacked with *any* illness during the last fifteen months. Not any have died; but all resumed their work in a day or two.

JOHN DOWLEY, Surveyor.

Supplement to the preceding Reports; being a Statement of the Health of the Workmen employed by Mr. James Creevy, *Nightman, of Drury Lane, December* 15, 1831.

A.—Sixty-six years of age; has been continually employed at night-work for thirty-three years; twenty years in the employment of Mrs. Garner of Mount Pleasant, Gray's Inn Lane, and thirteen years with his present employer, Mr. Creevy. [Never ill at his work; a very hard drinker.]

B.—Thirty-five years of age; nine years in Mr. Creevy's employ. [Never ill at his work; very drunken.]

C.—Twenty-seven years of age; ten years in Mr. Creevy's employ. [Never ill at his work; moderate drinker.]

D.—Forty-five years of age; eight years in Mr. Creevy's employ; was seven years in the navy; is running driver to the Phœnix engine. [Never ill at his work; drinks hard.]

E.—Thirty years of age; six years in Mr. Creevy's employ. [Never ill at his work; a great eater, and a very hard drinker.] —This man is employed five nights a-week, and is always employed in drawing up the soil, which is considered the worst part of the work.

F.—Twenty-three years of age; four years in Mr. Creevy's

employ. [Never ill at his work; drinks hard at night-work, but not in the day.]

G.—Thirty years of age; five years in Mr. Creevy's employ. [Never ill at his work; drinks moderately.]

H.—Thirty-five years of age; six years in Mr. Creevy's employ. [Never ill at his work; drinks moderately.]

I.—Sixty years of age; seven years in Mr. Creevy's employ. [Never ill at his work; hard drinker.]

Mr. Creevy has been twenty-five years in business; never knew any of his men to be ill at the regular night-work; almost all of his men have been blind for a day or two, after cleansing very foul privies or barrack privies.

Mr. Creevy says his workmen all drink gin, and smoke tobacco, at night-work; and that he thinks nightmen have better health than most labouring men. Mr. Creevy says he was employed, about six weeks ago, to empty two cesspools, or receptacles for decomposed flesh, at the anatomical school in Windmill-street. All his men came home sick; he had them all washed with warm water, and made them gargle their throats with warm water and vinegar, except G., who went home without taking this precaution; he was ill a week in consequence.

Mr. Creevy also states,—in answer to inquiries as to the nature of the blindness with which some of his men have been afflicted after emptying very foul privies, as stated above,—that from a practice of twenty years, he finds the result nearly as follows:—

Large cesspools, such as cesspools of workhouses, and cesspools of barracks of horse and foot guards.

The man employed with the pail standing over, and the man at the bottom, have returned home in the morning with their eyes in a strong state of inflammation*; after washing they have applied a cold lotion, and have been able to return to their employment, after twenty-four hours, without the aid of medical assistance, as if nothing had occurred; and Mr. Creevy has never found any unfavourable symptoms to supervene.

* I have seen some of these cases, and find the inflammation resembles erysipelas.—A. C.

LXIII. *Reviews, and Notices respecting New Books.*

Comparative Account: Population of Great Britain. Ordered by the House of Commons to be printed, 19*th October,* 1831.

[Concluded, from p. 220.]

NO country but Great Britain exhibits so much that is truly interesting in all that regards calculations relating to the duration of human life. On the right hand and on the left, institutions are reared to protect the orphan and the widow. Much light remains, however, to be thrown on this most interesting and deeply important subject. The parish registers have, in the census of 1831, been made subservient to this important end. The parish registers of England form, indeed, in the aggregate a vast national record; and, we may add, have hitherto been singularly neglected. The village clerk, when he records the baptism of a child, or performs the melancholy office of registering the dead, is unconsciously performing a duty acceptable to the philosopher, and silently creating materials to mitigate the sorrows of life. The earliest of these records date from the establishment—the venerable establishment—of the Church of England, injunctions to that effect having been issued by Cromwell (Henry's vicegerent for ecclesiastical jurisdiction) in 1538. These were repeated in the beginning of the reign of Elizabeth, who also appointed a protestation to be made by the clergy, in which among other things they promised to keep parish register books in a proper manner. In 1812, however, a new Parish Register Act was passed, and which during eighteen complete years has now produced the ages of four millions of the deceased. To extract from the entries in the parish registers this vast account, was committed to the charge of the clergy, and the work was in most cases performed by them personally. To arrange the resulting lists into aggregates for each year, and for every age, since 1812, will be a work of vast labour; but Mr. Rickman's unceasing industry and admirable arrangements will do this. He will be stimulated in his laborious inquiry by the recollection, that the materials on which he is employed are by far the most perfect of any hitherto submitted to investigation. The problem of the increased duration of life, which singularly enough has attracted more notice abroad than at home, will thus have new light thrown upon it. We wait impatiently for this part of Mr. Rickman's labours; and if we are correctly informed, he has already arrived at some singularly interesting results. It will form, without doubt, the most splendid contribution that has yet been made to the law of mortality.

The following table contains an account of the population of England, Wales, and Scotland, at each of the four periods of enumeration. The total amount for 1831 is 16,537,398. The population increased from 1811 to 1821 at the rate of 17[illegible] per cent., but from 1821 to 1831, only $15\frac{1}{2}$ per cent.

POPULATION.

	1801.	Incr. per cent.	1811.	Incr. per cent.	1821.	Incr. per cent.	1831.	1831. Males.	1831. Females.
England & Wales	8,872,980	$14\frac{1}{2}$	10,163,676	$17\frac{3}{4}$	11,978,075	16	13,894,574	6,769,469	7,125,105
Scotland...	1,599,668	13	1,805,688	16	2,093,456	13	2,365,807	1,115,132	1,250,675
Gr. Brit. collecty	10,472,048	$14\frac{1}{3}$	11,969,364	$17\frac{2}{3}$	14,072,331	$15\frac{1}{2}$	16,260,381	7,884,601	8,375,780
Army, Navy, &c.......	470,598		640,500		319,300		277,017	277,017	
	10,942,646	$15\frac{1}{4}$	12,609,864	14	14,391,631		16,537,398	8,161,618	8,375,780

Of the rates of increase of the English counties during the ten years from 1821 to 1831, Monmouth stands at the head, being 36 per cent. Lancaster is 27 per cent. The lowest increment is 2 per cent. for the North Riding of York. Of Wales, Glamorgan presents a maximum of 24 per cent.; and Merioneth a minimum of 3 per cent. In Scotland, Lanark affords a maximum of 30 per cent., and Berwick and Selkirk, minima of 2 per cent.—We give in the next table the amount of population for each of the English, Welsh and Scotch counties in 1821 and 1831, together with the rates of increase per cent. during that time. The causes are singular, which accelerate population with so much rapidity in some counties, and so slowly in others.

ENGLAND.

Counties of	1821.	Incr. per cent.	1831.	*Counties of*	1821.	Incr. per cent.	1831.
Bedford	83,716	14	95,383	Northampton	162,483	10	179,276
Berks.	131,977	10	145,289	Northumberland	198,965	12	222,912
Buckingham	134,068	9	146,529	Nottingham......	186,873	20	225,320
Cambridge	121,909	18	143,955	Oxford............	136,971	11	151,726
Chester	270,098	24	334,410	Rutland...........	18,487	5	19,385
Cornwall	277,447	17	302,440	Salop	206,153	8	222,503
Cumberland......	156,124	10	169,681	Somerset..........	355,314	13	403,908
Derby	213,333	11	237,170	Southampton	283,298	11	314,313
Devon	439,040	13	494,168	Stafford	345,895	19	410,485
Dorset	144,499	10	159,252	Suffolk	270,542	9	296,304
Durham	207,673	22	253,827	Surrey	398,658	22	486,326
Essex	289,424	10	317,233	Sussex	233,019	17	272,328
Gloucester	335,843	15	386,904	Warwick	274,392	23	336,988
Hereford..........	103,243	7	110,976	Westmoreland...	51,359	7	55,041
Hertford..........	129,711	10	143,341	Wilts.	222,157	8	239,181
Huntingdon......	48,771	9	53,149	Worcester	184,424	15	211,356
Kent	426,016	12	479,155	York (E. Riding)	154,010	10	168,646
Lancaster.........	1,052,859	27	1,336,854	City of York and Ainstey	30,451	17	35,362
Leicester..........	174,571	13	197,003				
Lincoln	283,058	12	317,244	York (N. Riding)	187,452	2	190,873
Middlesex	1,144,531	19	1,358,541	York (W. Riding)	801,274	22	976,415
Monmouth	71,833	36	98,130				
Norfolk	344,368	13	390,054		11,261,437	16	13,089,338

WALES.

Counties of	1821.	Incr. per cent.	1831.
Anglesea	45,063	7	48,325
Brecon	43,603	10	47,763
Cardigan	57,784	10	64,780
Carmarthen	90,239	12	100,655
Carnarvon	57,958	15	65,753
Denbigh	76,511	8	83,167
Flint	53,784	11	60,012
Glamorgan	101,737	24	126,612
Merioneth	34,382	3	35,609
Montgomery	59,899	9	66,485
Pembroke	74,009	9	81,424
Radnor	22,449	9	24,651
	717,438	12	805,236

SCOTLAND.

Counties of	1821.	Incr. per cent.	1831.
Aberdeen	155,387	14	177,651
Argyle	97,316	4	101,425
Ayr	127,299	14	145,055
Banff	43,561	12	48,604
Berwick	33,385	2	34,048
Bute	13,797	3	14,151
Caithness	30,238	14	34,529
Clackmannan	13,263	11	14,729
Dumbarton	27,317	22	33,211
Dumfries	70,878	4	73,770
Edinburgh	191,514	15	219,592
Elgin	31,162	10	34,231
Fife	114,556	12	128,839
Forfar	113,430	23	139,606
Haddington	35,127	3	36,145
Inverness	90,157	5	94,797
Kincardine	29,118	8	31,431
Kinross	7,762	17	9,072
Kircudbright	38,903	4	40,590
Lanark	244,387	30	316,819
Linlithgow	22,685	3	23,291
Nairn	9,006	4	9,354
Orkney and Shetland	53,124	10	58,239
Peebles	10,046	5	10,578
Perth	139,050	3	142,894
Renfrew	112,175	19	133,443
Ross & Cromarty	68,828	9	74,820
Roxburgh	40,892	7	43,663
Selkirk	6,637	2	6,833
Stirling	65,376	11	72,621
Sutherland	23,840	7	25,518
Wigtown	33,240	9	36,258
	2,093,456	13	2,365,807

SUMMARY OF GREAT BRITAIN.

England	11,261,437	16	13,089,338
Wales	717,438	12	805,236
Scotland	2,093,456	13	2,365,807
Army, Navy, &c.	319,300		277,017
	14,391,631	15	16,537,398

It has been objected, and properly too, by different writers on Population, that there is so much vicissitude and uncertainty connected with the male part of every community, and particularly in a busy and enterprising people like our own, that the ratio of augmentation cannot be fairly obtained, unless that portion of the inhabitants be omitted. The increase of the female sex has hence been regarded as a better test, and Mr. Rickman has prepared the following table to that effect, thereby virtually omitting, throughout the calculation, such of the army, navy, and merchant-seamen as were not domiciled in Great Britain.

1801. Females.	Increase per cent.	1811. Females.	Increase per cent.	1821. Females.	Increase per cent.	1831. Females.
5,492,354	14·15	6,269,650	15·71	7,254,613	15·45	8,375,780

But the most remarkable portion of this comparative account is that relating to London. The metropolis of the British empire is truly the wonder of the whole world. Its population, amounting to nearly a million and a half, discloses a vast field for contemplation, both to the political and the moral philosopher. As a great centre of energy, its influences are not only powerfully felt in every part of our European empire, but the impulse is transported through a dominion, which the sun in some region or other perpetually gilds! It is a great moral point of action to which every eye is turned; and the mighty energies it displays surpasses everything in ancient or modern times. In literature, the sciences, the liberal and useful arts, in associations honourable to humanity, in the political energies it can exert when roused into action;—the lofty examples of virtue, and the vice and profligacy which it displays,—what a mighty and tremendous subject for contemplation! For such a city to be augmenting its population at the rate of 20 per cent., in the short interval of ten years, who will venture to estimate its immense physical and moral energies at the end of the present century! Surely this alone is an application of the labours of the statistical philosopher, which cannot but recommend them to the attention of mankind. Such a population, and above all, a reading, calculating and reflecting population, concentrated within the limits of a few square miles, what an influence it must exert on the destinies of the whole world!

London within the walls, properly so called, is the parent city, around which this mighty metropolis has spread itself in all directions. "No place in Great Britain," Mr. Rickman observes, "can have been an earlier resort of commerce, the name of London occurring as a celebrated mart* before the Romans had subdued the natives into steady obedience. The situation of London was no doubt selected as being at the head of a navigable tideway, the deep water ceasing at London Bridge, and the river not being navigable for sea-borne vessels over the Vauxhall Shoal. London is thus placed fifty miles inland, an advantage more striking, as although England is not extensive enough to produce a large river, such access of shipping is unequalled (except perhaps by the Elbe) on the continent of Europe. This situation has always secured to the merchants of London the supply and the export of a territory not less than 300 miles in circumference; and the superior power of assortment at such an emporium, has always enlarged their commerce in a greater proportion than this fortunate position naturally indicates. The unembanked Thames must have appeared as an estuary of some breadth, in which the same quantity of tidal water could have had comparatively little effect; and the hill of moderate acclivity, on which the City of London within the walls is placed, must have been more remarkable and conspicuous than at present. From the eastern ascent at Tower Hill to the western descent at Ludgate Hill, its extent exceeds an English mile; and the walls extend to the northward so as to inclose a space more than three

* Tacitus, Ann. lib. 14. Londinium, cognomento quidem, coloniæ non insigne, sed copiâ negotiatorum et commentuum maxime celebre. [A.D. 61.]

miles in circuit. The walls of London are of Roman foundation; and the population crowded within them," continues Mr. Rickman, "would now be justly deemed excessive, as was proved by frequent pestilence and an unusual rate of mortality at all times; but the great fire which consumed more than the entire city *within the walls* in the year 1666, seems to have precluded pestilence in the renovated city*."

In the beginning of the last century, Mr. Rickman considers the population to have been not much less than 140,000, as proved by deduction from the parish registers, and the annual mortality as one in twenty. Fortunately for the health of the citizens, space is become more valuable for warehouses than for human habitation; so that the population of the city within the walls is diminished to 55,778, and the rate of mortality to less than one in forty.

The City of London *without the walls* has been acquired by successive royal grants of jurisdiction. The main part of it extends westward to Temple Bar, constituting the best built part of the town in the reigns of the Plantagenets. The population of this portion of the metropolis was about 69,000 at the beginning of the last century, but it now amounts only to 66,209.

The Borough of Southwark has been repeatedly granted to the City of London, of which it forms the Bridge-ward without; but the jurisdiction of the City has always been resisted in the Borough. Its origin cannot but be ascribed to the ferry, which in Roman times connected London with the military road to Dover. The Roman roads were all measured from London Stone †, still extant in Cannon-street, from whence the road passed immediately down to the *Water Gate* of the city, the ferry crossing to the end of a causeway (now Bank-end),

* At the moment we write this, we fear this will not be strictly verified with regard to the cholera; but its ravages are less virulent there than in other parts.

† No defect in the metropolis is more inconvenient than the want of such a stone, the various roads from London being now measured from ten or eleven different places, two, three, and even four miles distant from each other. The catalogue is curious: Hyde Park Corner and Whitechapel Church; the Surrey side of London Bridge and of Westminster Bridge; Shoreditch Church, Tyburn Turnpike; Holborn Bars (long since removed), "the place where St. Giles's Pound formerly stood"; "the place where Hicks's Hall formerly stood;" the Standard in Cornhill (of which no other tradition remains, its exact site being unknown); and the "Stones' End in the Borough", which moves with the extension of the pavement. Thus the actual distance of any place cannot be known without minute inquiry and local knowledge of London. The easy remedy consists in adopting the mileage of the Post Office, when it shall have been remeasured from the new site of that office, the frontage of which grand centre of communication could not be more appropriately adorned, than by an obelisk, which would become a *London Stone*, in imitation of that which stood in the Forum of ancient Rome. The vicinity of St. Paul's, the most conspicuous object in London, recommends the new Post Office especially for this purpose; and turnpike road trustees would not refuse to accommodate to it their milestones, under the direction of the road surveyor of the Post Office.

pointing to St. George's Church; from whence the line of the Roman road is still in use. Mr. Rickman has here thrown some light upon an interesting period of our history. After the death of Sweyn, the Danish invader, who had expelled King Ethelred from England, Ethelred obtained the aid of Olaf, chieftain of a band of Northern adventurers, and attacked the Danes, who were then in possession of London. Olaf's fleet, however, was found to be of little use, unless it could pass the fortified bridge, then of wood, and wide enough for the passage of two carriages. The bridge was defended at its *south* end by a military *work*, placed in what the historian calls the great emporium of Southwark. The first attack on the bridge having failed, Olaf proceeded to fit his ships with a bulwark, and under this cover, fastened them to the legs of the tressels, or timber supports of the bridge. His rowers, taking advantage of the current, tore away the middle of it; and the Danes were in consequence subdued. A few years afterwards Canute invaded England, and attempted to pass the repaired bridge; but due precaution had been used in re-constructing it, and Canute was driven to the necessity of digging a canal, and passing his fleet outside of the Southwark fortress. These two attempts on London Bridge are scarcely mentioned in our History; and in truth because they have been confounded together, notwithstanding the obvious absurdity of supposing that Canute dug a canal, when he had removed the obstacle which rendered such canal necessary. Moreover, the Danes were defendants in the first instance, and assailants in the second. The first attempt succeeded, but the second failed; nor in the opinion of the best critics in Danish history did Olaf ever cooperate with Canute. "The narrative of these remarkable instances of military resource must henceforth," observes Mr. Rickman, "take a place in our History;" and we regret that our limits prevent us from introducing it here. We thus see how the diligent cultivator of statistics may aid the antiquarian and the historian in their interesting and ennobling pursuits. The population of Southwark was 45,000 at the beginning of the last century, and at present amounts to 91,500.

The population of Westminster was 130,000 at the commencement of the last century, but at present it amounts to 202,050.

The Bills of Mortality, from which the fifth division of the metropolis is designated, is very minutely described by Mr. Rickman. "London," he says, "used always to suffer heavily from the plague; and in the great pestilence which, originating in the East in 1345, reached England in 1348, it seems well established that 100,000 persons died and were buried in the city*. In 1563 above 20,000 persons died

* It is said that by far the greater part of mankind were swept away by this Indian pestilence, which ravaged Asia, Africa and Europe in succession. Joshua Barnes, in his Life of Edward III., (p. 428—442.) seems to have collected all the truth and all the exaggeration which reached posterity on this subject. From the indisputable fact, that few persons of rank or condition died, it may be inferred, either that wholesome diet, sufficient clothing, and personal cleanliness operated as preservatives against this disease;

of the plague; in 1592 above 15,000; and in 1603 more than 36,000. This frequent recurrence caused the establishment of notices, called Weekly Bills of Mortality, which were kept and published by the parish clerks, as a warning to the Court and to others to leave London, whenever the plague became more fatal than usual. In the year 1625, above 35,000 persons died of the plague; in the year 1636 above 10,000; and 68,596 persons died in the last great plague of 1665. The conflagration which destroyed the whole city occurred in the year 1666, after which the plague languished, and finally disappeared from the Bills of Mortality in 1679. The somewhat obsolete names of diseases in these Bills have," says Mr. Rickman, "injured their reputation;" but we are disposed to censure them further. The London Bills of Mortality have too long remained stationary, too long retained the barbarous language and forms in which they were originally delivered. Ought this to be so? They are perpetually referred to by able writers, notwithstanding it is known that much of the evidence they contain is derived from the testimony of old women. Major Graunt, above a century ago, urged with the greatest truth, that "the old women searchers, after the mist of a cup of ale, and the bribe of a two-groat fee, reported those to die of consumption, who really died of diseases of a very different kind." Mr. Rickman mentions that the Bills of Mortality are discontinued in some of the larger parishes. We are sorry for this. We would not have them abandoned, but improved, and brought into perfect keeping with the general spirit of the age. We are unquestionably behind most other civilized nations in the subject of registers. Sweden, for example, has long possessed a well-digested system of medical statistics; but "England," as Sir Walter Scott once eloquently observed, "which has commanded arts, sciences and manufactures to arise, as the rod of the prophet produced waters in the desert,"—has singularly neglected the important subject of medical statistics. We forbear, however, dilating on this at present, it being our intention to treat of it with more fullness in our review of Hawkins's Medical Statistics, in a future Number. Within the limits of what is called the Bills of Mortality, the population was 326,000, in the beginning of the last century, but it now amounts to 760,000.

A few parishes not within the Bills of Mortality, but adjoining the metropolis, form the last division. In the early part of the past century, these were but thinly peopled, containing only 9,150 persons, occupying a few scattered hamlets, and living in a manner comparatively rural. At the present moment these same parishes are filled with an active and intelligent community amounting to 293,560 souls.

These are the six great parts into which Mr. Rickman has divided our metropolis. Its whole population was 674,000 at the commencement of the last century, but in 1831 it amounted to upwards of 1,500,000, including the usual allowance for seamen and strangers. This gives an increase of 222 per cent., while the population of the

or that the alleged mortality is enormously exaggerated, although that in London is supported by circumstantial evidence, which appears to be conclusive.

whole of Great Britain has been augmented from 5,475,000 to 13,888,000, or 254 per cent., in the same time. The whole population of the kingdom has increased, therefore, with still greater celerity than that of the metropolis. The total number of inhabitants of all the parishes, whose churches are situated within eight English miles, measured directly from St. Paul's Cathedral, amounted to 1,031,500 in 1801; to 1,220,200 in 1811; to 1,481,500 in 1821; and in 1831 to 1,776,556, or to *more than one million and three quarters*, a twenty-fifth part being added in each case as a moderate allowance for the great number of British seamen belonging to the registered shipping at anchor in the river Thames, for soldiers quartered in the Tower, and various other barracks, and for the transitory population, always arriving and departing so irregularly as to prevent their enumeration, in a city where no police regulations exist respecting strangers and occasional residents. We may remark, in order to illustrate this enormous population, that the whole number of inhabitants of the great county of York amounts but to 1,371,296; and if to this be added the entire amount of the county of Warwick (336,988), the aggregate will still fall short, by nearly 70,000, of the souls actually existing in the metropolis, within the limits alluded to. Let us, therefore, imagine *all* the inhabitants of these two counties driven by some common impulse to assemble near the hills of Severus;—the people of Leeds, Sheffield, and Hull;—all the busy population of the Wapentakes of Agbrigg, Morley, Strafforth and Tickhill;—the innumerable townships, the sokes and the boroughs: in a word, the whole of the inhabitants of the three great Ridings of York;—Birmingham too, and the many parishes and hamlets of Warwick,—an awfully immense multitude, animated by a common feeling, but still the mighty aggregate *less* than the total amount of London! The mind, when attempting to form a scale of this sort, to aid its feeble energies, becomes indeed overwhelmed with the consideration. Or could we, to put the subject in another point of view, visit during the present year the different towns and cities of the empire,—traverse our modern Athens, the immense cotton manufactories of Manchester and Glasgow;—Birmingham, converting every metal to a useful purpose;—Leeds with all its woollens;—Norwich with its crapes, and Nottingham with its stockings; and afterwards move on to the great commercial seaports of Liverpool, Bristol, New and Old Aberdeen, Newcastle, Hull, and Dundee; or lastly pass to the two great naval arsenals of Plymouth and Portsmouth, where the wooden walls of Old England are seen reposing, as Mr. Canning once eloquently observed, on their shadows; —and could we gather together in one great mass all the inhabitants of these different towns,—the old and the young,—men, women and children of every rank and denomination, the overwhelming assemblage would still fall short, by above two hundred thousand souls, of the immense population of London. Surely the labours of the statistical inquirer are not useless, when they unfold to us realities like these.

In order to compare London with Paris, Mr. Rickman has taken the population of the department of the Seine, as included in a district

nearly circular, 16 miles in diameter. This amounted to 637,000 in 1818, to 742,000 in 1820, and to 1,013,000 in 1829 *.

While London far surpasses any other capital city in the amount of its inhabitants, and we would add in its wealth, industry and general intelligence, England exceeds every other country in the magnitude and growth of its great towns. This would be an interesting subject to touch upon, but our limits warn us of the necessity of closing our review. We insert the population of London, together with those of the great towns, in 1821 and 1831, in the succeeding table, distinguishing also the males and females.

Towns.	1821.	Incr. per cent.	1831.	1831. Males.	1831. Females.
London, within the Walls } City	56,174	3	57,695	28,626	29,069
London, without the Walls, (including the Inns of Court) } City	69,260		67,878	33,401	34,477
Southwark, Borough	85,905	7	91,501	44,318	47,183
Westminster, City	182,085	11	202,080	95,314	106,766
Par. within the Bills of Mortality.	616,628	23	761,348	354,253	407,095
Adjacent Par. not within the Bills	215,642	36	293,567	128,529	165,038
Metropolis	1,225,694	20	1,474,069	684,441	789,628
Edinburgh, City	138,235	18	162,403	72,515	89,888
Manchester, Salford, and Suburbs	161,635	47	237,832	112,873	124,959
Glasgow (and Suburbs) City	147,043	38	202,426	93,724	108,702
Birmingham (and Suburbs)	106,721	33	142,251	69,415	72,836
Norwich, City	50,288	22	61,116	27,671	33,355
Paisley, with the Abbey Parish	47,003	22	57,466	26,522	30,944
Nottingham, Town	40,415	25	50,680	23,616	27,064
Liverpool (with Toxteth Park) Bor.	131,801	44	189,244	87,919	101,323
Bristol (with Suburbs) City	87,779	18	103,886	46,525	57,351
Aberdeen, New and Old	44,796	30	58,019	25,235	32,784
Newcastle-upon-Tyne (with Gateshead,) Town.	46,948	23	57,937	26,951	30,986
Hull (with Sculcoates) Town	41,874	18	49,461	22,288	27,473
Dundee	30,575	48	45,355	20,810	24,545
Plymouth, Devonport, & Stonehouse, Borough	61,212	23	75,534	33,043	42,491
Portsmouth, Portsea, & Gosport, B.	56,620	11	63,026	27,737	35,289

Here we must positively close, although much remains to be said. To Mr. Rickman, the philosopher—the lover of human kind, whoever he may be, cannot but feel indebted; and we all must feel ardently

* See the *Moniteur* (*partie officielle*), 29th August, 1818; 10th March, 1819, and 5th February, 1829. The first two of these estimates evidently do not include resident foreigners and the inhabitants of the provinces resident in Paris, who by comparison with a non-official return from the *Bureau des Longitudes* (14th December, 1818) appear to have amounted to 149,000 persons.

desirous that the other important labours on which he is engaged may be speedily completed. Of Mr. Rickman's devotion to these pursuits we have had too many proofs to permit us to suppose that he will ever relinquish that which he has begun. The public eye is fixed upon his labours, and the gratitude of every lover of statistics awaits him. He has set a splendid example for another and another generation to follow.

Dr. PEARSON'S *Introduction to Practical Astronomy.* 4to. 2 vols.

It has been observed, and with much truth, that a great book is a great evil. To those numerous works of the present day which have their bulk enormously increased by a redundancy of uninteresting and superfluous matter, the severe censure implied in this observation cannot be too unmercifully applied. When we were favoured with a copy of the work under consideration for our perusal, we were under some apprehension that it might be subject to the above censure. It professes to be an "Introduction to Practical Astronomy;" and when we contrasted the small compass into which the few existing treatises on this subject are compressed, with the bulkiness of these volumes by Dr. Pearson, we were fearful that the extent of the work was more than commensurate with the subject.

A very hasty glance over the contents of the pages, however, quickly convinced us that we were greatly in error, and induced us to give the whole work that strict and attentive examination which it so justly demands, and from which alone its merits can be duly appreciated.

The volumes before us do not consist of an oppressive accumulation of extraneous and redundant matter, but of an extensive collection of every thing that is curious and valuable in the history and practice of the interesting science of which they treat: and in addition to this valuable concentration of the labours of others, there will also be found a large mass of original matter resulting from the long practical experience of the learned and ingenious author; who has spared no expense in furnishing himself with the very best instruments that human ingenuity can contrive, and no labour in conducting experiments and examining results tending in any degree to illustrate the subject.

The author has given to this work the modest title of being merely an "Introduction to Practical Astronomy." We must, however, pronounce it to be much more. It not only brings us into the subject, but leads us most carefully through all its intricacies; nor is there any part in the practical department that is not most completely explained and illustrated.

To do justice to this subject manifestly requires a person not only of considerable talent, but of long experience in the management of instruments. No person could have been found better qualified in every respect for this undertaking than our author; and we have great reason to congratulate ourselves that his inclinations have led him to labour in this troublesome department of science, where so little has been done, and where so much was wanted.

There is no mechanical art in which so much delicacy and so much ingenuity are required as in the construction of astronomical instruments. The mechanical turn of our author's mind peculiarly qualifies him for treating of the proper management of this extremely delicate species of mechanism. It seems to have long been a source of peculiar delight to him, and of never-failing amusement. If we refer to Rees's Cyclopædia, we shall there find large contributions from the pen of our author, consisting of the description of various pieces of horological and planetary mechanism, as well as of astronomical instruments; but his mechanical genius is more particularly developed in the description of some instruments of his own contrivance. We refer to the construction of some extremely ingenious planetaria, in which the motions are conducted by trains of mechanism which exhibit an epitome of the celestial motions in the most striking and accurate manner: these, however, may perhaps be considered as astronomical toys, but they are the prettiest toys of the kind we have ever seen; and for those who are but *children* in the science, are well calculated to convey a correct notion of the complicated celestial motions which they represent.

But further than this, the author has long been habituated to the use of instruments of a complicated construction, requiring the nicest adjustments; for we understand that most of the instruments described in this work have been long in his own possession, and in constant use; and this long practical experience qualifies him duly to appreciate the value even of those instruments he has never seen. His account of an instrument, and his suggestions for its improvement are not those of a mere enthusiastic theorist, who overlooking all intervening objections comes at once to his favourite conclusion. Such an one, for example, might imagine, and many have imagined, that in the construction of instruments the *larger* they are made the *better;* and that the increased power of the reading microscopes, combined with the additional power of the telescope and the enlarged divisions on the limb, would enable him to read off an angle with *certainty* to the *tenth* part of a second. But a person who, like our author, has had much experience in the use of instruments, will reason otherwise, and will come to a different conclusion. He will take into his consideration the imperfection of the materials necessary for constructing the instrument, the variable temperature to which the different parts of a large vertical circle (for example) are exposed: and he will be led to conclude that errors may, and probably will, be induced by these various unavoidable circumstances, which will more than counterbalance the advantage derived from the increased dimensions of the circle. These sources of error must of necessity limit the maximum size for an instrument, and will in our opinion reduce this maximum much below what it is generally imagined to be. It is our firm belief that a ten-feet mural circle* would so far exceed the legitimate maxi-

* Ramsden constructed the Dublin circle, of ten feet diameter, and afterwards reduced it successively to nine and eight feet, the latter of which is its present size.

mum, as to be almost, if not entirely, useless: indeed we apprehend that the six-feet circles at the Royal Observatory are too large; and are firmly persuaded that two five-feet circles, with an increased power of the telescope and of the microscopes, would give more unvarying results than the larger instruments now used at Greenwich.

The work before us possesses additional interest, and derives additional value from the well-earned tribute of praise which it offers to the first astronomical instrument-maker of the present or of any former age. Mr. Troughton stands quite unrivalled in the construction of original astronomical instruments; and from his peculiar talent they have acquired a degree of perfection that, with the present means and materials of construction, will probably never be surpassed. Indeed it is not easy to conceive that greater precision can ever be attained in dividing the limb of a large circle, than is insured by the application of the method of optical division which has been described by the inventor in the Philosophical Transactions, and by our author in Rees's Cyclopædia. There are many other excellent artists in London, particularly T. Jones of Charing-Cross, who was a pupil of the celebrated Ramsden, and Simms, who is now Mr. Troughton's partner; and we feel confident that these will not think it any disparagement to themselves, when we assert that Troughton does, and we believe always will, hold that rank among the makers of astronomical instruments that Sir Isaac Newton does among philosophers. To Mr. Troughton it must be peculiarly gratifying to find the second volume of this work dedicated to him, in which he makes so conspicuous a figure; and in which is found a history of several of his works, the existence of which perhaps might not otherwise have been known, except at the different observatories in which they are deposited. To Troughton this distinction was justly due; and we cannot forbear admiring our author's good taste in paying him this tribute of respect. To several other eminent artists in this department he has done full justice; and they also must feel much gratification from the circumstance of having the results of their ingenuity so ably and minutely described, by a person so well capable of appreciating their merits, and of bestowing that praise upon them which they have justly deserved.

A work of this kind has long been a desideratum with persons desiring to perfect themselves in the *practice* of astronomy. For when we look at the few works which are extant on the subject, particularly in our own language, we really find nothing at all that is adapted to the present improved state of astronomical science. On this account the want has long been felt of some work, giving a comprehensive detail of all the principal instruments in use, and also of the precautions necessary to be adopted; in order that such instruments may be rendered as effective as possible. This is the more requisite in the present state of the science, when errors, which would have formerly passed without detection, or if detected, would have been considered insignificant, are now of so much importance as to vitiate every result into which they are admitted.

Moreover, the precision in the construction of astronomical instruments is now brought to so high a degree of perfection, that it requires most cautious and delicate treatment in the use of them to do them justice. In the construction of the old instruments, in which the imperfections resulting from inaccurate divisions, or bad workmanship in general, were much greater than those resulting from the want of proper precautions on the part of the observer, these last were not likely to be detected : indeed it would have been useless to attempt to detect them ; for so long as they were less than the instrumental errors, their application, as equations, must have been in all cases vague and uncertain. The main object was then, as it must be *now*, to use such precautions as will keep the errors of observation below the errors of construction; and this, perhaps, in the old instruments, such as the great mural quadrants, was not difficult to effect. But the case is far different with instruments of modern date and construction; for should an observer be furnished with instruments of the most perfect workmanship, and should he, from not taking the proper precautions in making his observations, introduce errors which are of greater magnitude than the quantities which are the objects of his search, all his labours must prove worse than worthless. The perfection of modern instruments is such as to require the greatest care and skill on the part of the observer, to do full justice to the powers of his instrument. As an illustration of this statement we may refer to the controversy on the subject of annual parallax in certain fixed stars, which was for some time warmly kept up between the present Astronomer Royal, and the late Professor of Astronomy in Dublin: nor is the question yet settled; nor need this be a matter of surprise, when we reflect that the absolute quantity in dispute lies within *a single second* of space*!

In order that any instrument may have its full powers brought into action in the best manner, it is necessary that extreme caution should be used by the observer : for, independently of errors arising from the want of due *adjustment* of the instrument, there are errors originating in the observer *himself*, which a person accustomed to make observations will readily understand. Another source of error arises from imperfect division of the limb ; but in modern instruments this is *almost* annihilated : yet, minute as it remains, it is necessary that the observer should keep the error resulting from the first sources *less* than this last-mentioned error. To do this requires all his powers and attention : he must direct every effort towards precision in completing the requisite adjustments, and also in making his observations ; so that the errors originating in *himself* may generally, or always if possible, be less than the errors of his instrument. Unless he does this, however perfect the instrument may be, he can never do full justice to its powers; a less perfect instru-

* Mr. Pond's paper on the parallax of *α Lyræ*, in which part of this discussion is contained, will be found in Phil. Mag. vol. lxii. p. 292; and further information on the subject at p. 452 and 466 of the same volume.

ment in his hands would be quite as effective: he precludes the possibility of detecting any instrumental errors, and of increasing by that means the correctness of his observations.

We justly look with the greatest admiration at the discoveries of Bradley; yet these, considered *simply* in the light of inequalities, lie, as it were, at the surface of astronomical discovery. Far be it from us, however, to attempt to undervalue the importance of these discoveries, or the surprising ingenuity which effected them. All we mean to assert is, that the merit of the discovery does not consist in having observed these inequalities *merely as inequalities*, for these were large in amount, and must necessarily have been detected by a connected series of observations with instruments of tolerable precision: but it is the explanation of the phænomena that justly demands our admiration; nor can we withhold it, when we contemplate the masterly mind of Bradley, with steady and unerring sagacity, disentangling the various inequalities from the mass in which they were so intricately involved, and assigning to each its proper cause and proper value. It is the extraordinary ability displayed in the analysis, not only *qualitative*, but *quantitative*, of an assemblage of most complicated physical phænomena, that must always place Bradley in the very first rank of philosophers.

But the time is past when discoveries of this kind can be expected, since all the large inequalities are known by the discoveries of Bradley. If others still exist, they are of a more minute description, which are not appreciable with certainty even by modern instruments. But it is entirely with magnitudes of this almost inappreciable value that the astronomer has now to deal; and modern instruments, when used with care, seem to be sufficiently powerful to estimate magnitudes of this kind with some degree of precision. Until we can find materials for the construction of astronomical instruments, which are not at all, or much less, affected by external causes, than those now employed, we must in vain expect more accurate divisions, or greater stability of position, than have been effected by the skill of Troughton and of Jones.

The principal object deserving attention now seems to be to *increase the powers of the telescopes and microscopes*, while we *limit the magnitude* of the instrument. We are aware that many persons consider the objections to the magnitude of instruments as groundless; but we are convinced that *no person* who has been accustomed to the use of instruments, will look upon them in this light. Next to this our attention should be directed to *use* modern instruments so as to do full justice to the great powers they possess; but this, as has been observed, requires the greatest skill and the most attentive and delicate treatment. The want of a comprehensive detail of instructions for this purpose has long been felt by young astronomers; and this want has never been supplied until Dr. Pearson, with unbounded industry, and the greatest liberality in the expenditure both of money and time, has furnished a work, which in our opinion is fully adequate to the object he had in view.

It is only, however, from a careful perusal of the work itself, to

which we shall on every occasion refer our readers, that any just idea of its extensive merits can be obtained.

[To be continued.]

The Anatomy and Physiology of the Organ of Hearing; with Remarks on Congenital Deafness, the Diseases of the Ear, some Imperfections of the Organ of Speech, and the proper Treatment of these several Affections. By DAVID TOD, Member of the Royal College of Surgeons. London, 1832, 8vo, pp. 147: lithographs 3.

As we do not recollect any work hitherto published which treats exclusively of the Anatomy and Physiology of the Ear, we are induced to notice the treatise before us, although it is in the main of a professional nature. From what the author states in his prefatory remarks, it appears that he has devoted a considerable portion of his time to the investigation of the structure and œconomy of the Ear; and from the slight perusal which we have given his treatise, we are inclined to think that he must have paid considerable attention also to his subject.

The work commences with a descriptive account of the Anatomy of the Ear, which is treated at once minutely and concisely, and also with something of novelty. We say *novelty*, for in every anatomical treatise which we have read, it is stated that the bones of hearing (*Ossicula Auditus*) are moved by only three or four muscles; whereas Mr. Tod has described not less than eight or nine muscles belonging to them, with their respective actions; stating his opinion that it is from the various modifications of these actions that the mind is indebted for all the pleasures it receives through the functions of the ear. It is certainly surprising that so many important structures of the ear should have been overlooked by such acute anatomists as Morgagni, Scarpa, Sœmmerring, Valsalva, Monro, &c.; and that these distinguished physiologists should have contented themselves, when accounting for the numerous actions which the structures of the tympanum are capable of producing, with referring them to the oscillatory motions which they supposed the bones of the ear were susceptible of receiving, merely because they were aware that the muscles which they had demonstrated were totally inadequate to the performance of so many modified actions. This circumstance the author adverts to in the form of a note, p. 19, which we cannot do better than quote, as illustrating his views of the anatomy of the ear.

" Many will probably be inclined to doubt the existence of all the structures which I am about to describe, from their having escaped the notice of every one who has hitherto investigated the anatomy of the ear; but they can all be seen in the different preparations in my possession, and can readily be demonstrated in the ear of a child, when dissected in the way which I have recommended at the end of this anatomical description. That every structure or organ I describe as muscular is so in reality, is obvious, not only from its appearance in the recent bone, but also from the articulations of the bones admitting of their being moved with facility in

the direction of the different muscles, and in no other; from their having spaces, which allow them to contract in the direction of their fibres; from their being found invariably present, when dissected in a proper manner; and from the parts of the ossicula auditus to which they are inserted, having eminences and depressions like the corresponding parts of other bones. But these are not the only reasons we have for considering the different parts which have been described as muscles, and not as ligaments or mere membranes; for if the bones were not moved by contractile organs in directions coinciding with their articulations, what would be the use of the articulations of the ossicula auditus, or of their being fixed so particularly to one another, and at the same time lying in so loose a manner in the cavitas tympani? Let us take, for example, the head of the malleus, which is by far the largest and strongest, and perhaps the most important part of the bone;—what would be the use of that part, were it to lie in the fossa navicularis without having muscular textures pulling in those directions in which it is capable of being moved? Could it act with that nicety which we presume is necessary to convey the variety of phænomena to which the ear is sensible, otherwise than by the medium of delicate and sensitive muscles?"

In addition to the five proper muscles of the auricle, which preceding anatomists have enumerated, Mr. Tod describes (p. 4—5) the *Obliquus Auris* and the *Contractor Meatus* or *Trago-Helicus*, which he observes are as obvious as any of the former. The muscles of the *Ossicula* are, according to him, as follows (p. 20—22): 1. *Anterior Mallei;* 2. *Posterior Mallei*; 3. *Internus Manubrii Mallei*; 4. *Anterior Capitis Mallei;* 5. *Superior Capitis Mallei,* (the Ligamentum proprium teres of Sœmmerring); 6. *Obliquus Incudis Externus Posterior;* 7. *Triangularis Incudis* (noticed by Dr. W. Holder in Phil. Trans. vol. iii. *anno* 1668); 8. *Stapedius Posterior* olim *Stapedius;* and 9. *Musculus* vel *Structura Stapedius Inferior,* of which he remarks that he has not as yet been able to discover muscular fibres in it sufficiently clear to warrant his calling them determinately by that name, although it looks more like muscular texture than any other.

The following are the contents of this portion of the work:—

Anatomy of the Ear:—Of the External Portion—Eminences and Depressions—Fissures, Ligaments, and Muscles—Meatus Externus—Membrana Tympani:—Of the Internal Portion—Cavitas Tympani—Use of different parts—Eustachian Tubes—Membrana Propria Tympani—Ossicula Auditus—Muscles, Membranes and Ligaments—Motions of the Ossicula Auditus—Blood-vessels of the Tympanum—Labyrinth—Vestibulum—Semicircular Canals—Cochlea—Aquæductus Fallopii—Chorda Tympani—Portio Mollis—Coverings—Blood-vessels of the Labyrinth—Ossification of the Temporal Bone—Directions how to examine the structure of the Tympanum.

We subjoin the last article of this enumeration, as giving to anatomists the means of discovering, and demonstrating the muscles described by the author.

"Take the temporal bone of an infant or fœtus, and after injecting it, remove all the soft parts from its surface, keeping the membrana tympani entire; then introduce the point of a small scalpel between the os annulare and the petrous portion on the under surface, and separate the one gently from the other, so as to raise the two inferior thirds of the former about a quarter of an inch from the latter, and put a small piece of wood between them until the parts become quite dry. The different parts will then remain permanently *in situ*, only a little on the stretch. Again, take another temporal bone of the same age, and gradually remove the osseous shell which forms the upper part of the fossa navicularis, and the inferior part of the cavitas tympani, and put the preparation aside to dry. Every texture will then appear *in situ*, and in a state of integrity. By repeating these dissections with slight alterations, every structure will be demonstrated in a variety of ways." p. 35.

Mr. Tod next proceeds to discuss the respective functions of the different parts of the ear, in the following order:

Physiology of the Ear.—Of the Functions of the Auricle—Meatus Externus — Membrana Tympani — Ossicula Auditus — Chorda Tympani—Cavitas Tympani—Eustachian Tubes—Fenestra Ovalis and Rotunda—Labyrinth.

The author subsequently discusses the causes and the treatment of the several malformations and diseases to which the structures of this organ are liable. It has often been regretted that the members of a profession which has conferred the greatest benefits upon mankind, should have so much excluded from their investigations the pathology of congenital imperfections, and in particular of those which affect the structures of the ear, as to have caused the deaf and dumb to be educated merely as incurables, instead of being put also under a proper train of medical and surgical treatment. To so great an extent, indeed, has the system of excluding congenital imperfections from pathological research been carried, that the bare attempt of any individual to alleviate the symptoms arising from a deformity has been sufficient to hold him up to the ridicule of his brethren; a circumstance which probably has deterred many from devoting a portion of their time to the investigation of these phænomena; and has also tended greatly to the encouragement of empiricism. In the work before us is shown the possibility that many cases of congenital deafness and of auricular disease are susceptible of relief;—it may thus prove the means of conferring important benefits upon those who are, or may be, visited with maladies so distressing.

The work concludes with remarks on several imperfections of the organ of speech, which are of the same nature as those relating to the œconomy of the ear.

As a school book, we are persuaded that Mr. Tod's Treatise on the Ear must soon be in the hands of every medical student, of whose library it will afterwards form a volume, as a manual of the anatomy and physiology of that organ.

LXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

May 3.—A PAPER was read, entitled, "An Account of certain new Facts and Observations on the Production of Steam," by Jacob Perkins, Esq. Communicated by Ralph Watson, Esq. F.R.S.

Having observed that water on the surface of melted iron was very slowly affected by the heat, although it exploded violently when the same fused metal was dropped into it, the author made a series of experiments on the time required for the evaporation of the same quantity of water successively poured into a massive iron cup, at first raised to a white heat, and then gradually cooled by the addition and evaporation of the water. The first measures of water were longer in being evaporated than those subsequently added, in consequence of the reduction in the temperature of the iron, until this temperature reached what the author calls the *evaporating point*, when the water was suddenly thrown off in a dense cloud of steam. Below this temperature, the time required for the complete evaporation of the same measure of water became longer in proportion as the iron was cooler, until it fell below the boiling point. The author accounts for these results from the circumstance that when the metal is at the higher temperatures, the water placed on its surface is removed from contact with it by a stratum of interposed steam. From these and other experiments, he is led to infer the necessity of keeping water in close and constant contact with the heated metal in which it is contained, in order to obtain from it, in the shortest time, the greatest quantity of steam.

The reading of a Paper, entitled, "On certain Irregularities in the Magnetic Needle, produced by partial warmth, and the relations which appear to subsist between terrestrial Magnetism and the geological Structure and thermo-electrical Currents of the Earth," by Robert Were Fox, Esq. Communicated by Davies Gilbert, Esq. V.P.R.S.—was commenced.

May 10.—The reading of Mr. Fox's Paper was resumed and concluded.

May 17.—The reading of a Paper, entitled, "On Harriot's Astronomical Observations contained in his unpublished Manuscripts belonging to the Earl of Egremont," by Stephen Peter Rigaud, Esq. M.A. F.R.S. Savilian Professor of Astronomy in the University of Oxford,—was commenced.

May 24.—The reading of Professor Rigaud's Paper was resumed and concluded.

In the Memoirs of the Royal and Imperial Academy of Brussels, for the year 1788, the Baron de Zach published a paper on the planet Uranus, in a note to which he states that, in the summer of 1784, he found in the library of Lord Egremont at Petworth, some old manuscripts of the celebrated Thomas Harriot, which he alleges afforded proofs that he had observed the solar spots, and the satellites of Jupiter before Galileo. In the Berlin Ephemeris for 1788, Baron

Zach gave a full account of his alleged discovery, drawn up from Harriot's papers; an English translation of which was circulated in this country, and has been perpetuated by its being inserted in Dr. Hutton's Mathematical Dictionary. The author, having been entrusted by Lord Egremont with Harriot's original papers, has examined them with every attention he could apply to the subject, and gives in the present memoir the result of his inquiry.

The observations of Harriot on the spots on the sun, fill seventy-four half-sheets of foolscap, the first being dated December 8, 1610. These papers are in good preservation: the writing is clear, and the drawings well defined. Baron Zach says, that "he compared the corresponding ones with those observed by Galileo, and found betwixt them an exact agreement." This, the author shows, is very far from being the case, and he also brings evidence to prove that the discovery of the spots on the sun was made by Galileo at latest in the summer of the year 1610, and very probably in or before the month of July. He allows, however, that Harriot's observation in December of the same year, was the result of his own spontaneous curiosity.

The first observation made by Harriot of the satellites of Jupiter, has for date the 17th of October 1610. Those that follow, extend to the 26th of February 1612: they are clearly written out on thirteen half-sheets of foolscap. But, even by the statement of Baron Zach, Galileo discovered them on the 7th of January 1610; that is, nearly eight months before Harriot.

The author has detected many other material inaccuracies in the account given to the world by Baron Zach of Harriot's observations. He concludes, however, by observing that Harriot ought not to be deprived of the credit which is justly due to him, because a greater share has by some persons been claimed for him than he is justly entitled to. He himself made no pretensions to priority in the discoveries in question.

May 31.—The reading of a Paper, entitled, "On the Correction of a Pendulum for the reduction to a vacuum, together with Remarks on some Anomalies observed in Pendulum Experiments," by Francis Baily, Esq. F.R.S.,—was commenced.

June 7.—The reading of Mr. Baily's Paper on the Pendulum, was resumed and concluded.

The author observes, that in all the experiments hitherto made with the pendulum, a very important correction, depending on the influence of the circumambient air, has been omitted; and that the philosophical world is indebted to M. Bessel for having first drawn the attention of the public more immediately to this subject. For, although Newton evidently suspected that such an influence existed, and although the subject had been since fully discussed by the Chevalier du Buat, nearly 50 years ago, yet it does not appear that any of the distinguished individuals, employed by the different Governments in making experiments on the pendulum in more recent times, have had any notion that the effect of the air, on the moving body, was any other than that depending on its density; and consequently varying in amount according to the specific gravity of the metal of

which the pendulum might be composed. But M. Bessel has shown that a quantity of air is also set in motion by the pendulum (varying according to its form and construction), and thus a *compound pendulum* is in all cases produced, the specific gravity of which will be much less than that of the metal itself. M. Bessel's principal experiments for establishing the accuracy of this principle, were made with two spheres, about two inches in diameter, differing from each other very considerably in specific gravity, one being of brass, and the other of ivory, and each suspended by a fine steel wire. The author of the present paper, however, pursued another and a very different course for obtaining the same end: namely, by swinging the same pendulum first in free air, and afterwards in a highly rarefied medium, nearly approaching to a vacuum. From the difference in the results, he deduces a factor (denoted by n), by which the old, and hitherto received, correction must be multiplied in order to obtain the new and more accurate correction indicated by M. Bessel; and which, in the case of the two spheres above mentioned, is found by that author to be equal to 1·95.

But Mr. Baily, instead of confining himself to spheres of this size, and composed of these two substances only, has extended his inquiries to pendulums of various magnitudes, substances and forms. His first recorded experiment is on Borda's platina sphere, the diameter of which is 1·44 inches; and he found that the old correction must in this case be multiplied by 1·88 in order to obtain the true and accurate correction; or, in other words, that the old correction was but little more than half what it ought to be. The author then tried three other spheres of precisely the same diameter, but differing considerably in specific gravity: namely, lead, brass, and ivory, all of which gave nearly the same result; the mean of the whole being $n = 1·86$. He next proceeded to spheres of the size used by M. Bessel, made of three different substances, viz. lead, brass, and ivory. These gave a result (agreeing very well with each other,) somewhat smaller than the former; the mean of the whole being $n = 1·75$: thus showing that the factor for the additional correction is due to the form and magnitude of the moving body, and not to its weight or specific gravity. This last value, as the author observes, differs from that deduced by M. Bessel as above mentioned; but the cause of the discordance does not appear.

The author then shows the effect produced on cylinders of various kinds, both solid and hollow, and suspended in different ways,—on lenses, on cylindrical rods, on bars, on tubes, on convertible pendulums, and on several clock pendulums, amounting to upwards of 40 in number. The results of these experiments give in each case a different value for the factor n; and which appears to depend on the extent of surface, in proportion to the bulk of the body exposed to the direct action of the air when in motion: further experiments, however, are requisite to establish this point in a satisfactory manner*.

* Since this paper was read, the author has made a number of additional experiments on various other pendulums, which, by permission of the Council, will form part of the original paper; and from which he is

But, in the author's opinion, enough is shown to indicate the necessity and propriety of a revision and correction of all the experiments hitherto made with the pendulum, either for the determination of its absolute length, or for ascertaining the true figure of the earth; and that for this purpose, the true correction must be found from actual experiment in each particular case; since, with very few exceptions, it cannot be determined by any mathematical deduction.

Mr. Baily then proceeds to point out some singular discordances arising from the knife-edge mode of suspending the pendulum, where the *same* knife-edge and the *same* agate planes are employed. From which he is led to infer that the pendulum furnished with a knife-edge and agate planes, as at present constructed, is a very inadequate instrument for the delicate purposes for which it was originally intended; and that a more rigid examination of that part of the instrument is requisite, before we can rely with confidence on the accuracy of the results obtained by it.

Some anomalies are then pointed out in the magnitude of the arc of vibration, and some remarks offered on the supposed inadequacy of the usual formula for determining the correction for the arc; but the author considers it desirable that further experiments should be made for the more accurate determination of this point.

In conclusion, the author expresses a doubt of the rigid accuracy of the length of the seconds pendulum, as deduced from the recent experiments of Captain Sabine.

To the whole are appended tables exhibiting the details of all the experiments made by the author, and the corrresponding results.

A Paper was read, entitled, "Researches in Physical Astronomy," by John William Lubbock, Esq. V.P. and Treas. R.S.

The present paper contains some further developments of the theory of the moon, which are given at length, in order to save the trouble of the calculator, and to avoid the danger of mistake. The author remarks, that while it seems desirable, on the one hand, to introduce into the science of physical astronomy a greater degree of uniformity, by bringing to perfection a theory of the moon founded on the integration of the equations employed in the planetary theory, it is also no less important, on the other hand, to complete, in the latter, the method hitherto applied solely to the periodic inequalities. Hitherto those terms in the disturbing function which give rise to the secular inequalities, have been detached, and the stability of the system has been inferred by means of the integration of certain equations, which are linear when the higher powers of the eccentricities are neglected; and from considerations founded on the variation of the elliptic constants. But the author thinks that the stability of the system may be inferred also from the expressions which result at once from the direct integration of the differential equations. The

led to infer that, in the case of spheres, cylinders, and other bodies suspended by rods of different diameters, the value of the factor depends not only on the body appended to such rod, but that the rod itself has a considerable influence on the result, except it be a very fine wire; when its effect becomes merged in that of the appended body.

theory, he states, may be extended, without any analytical difficulty, to any power of the disturbing force, or of the eccentricities, admitting the convergence of the series; nor does it seem to be limited by the circumstance of the planet's moving in the same direction.

A Paper was also read, entitled, "On the Nervous System of the *Sphinx Ligustri* (Linn.), and on the Changes which it undergoes during a part of the Metamorphoses of the Insect," by George Newport, Esq. Communicated by Peter Mark Roget, M.D. Sec. R.S.

The author gives a minute anatomical description, accompanied by drawings, of the development and arrangement of the nerves of the *Sphinx Ligustri*, and the successive changes they undergo during the last stage of the larva, and the earlier stages of the pupa state. As this insect, in passing from its larva to its perfect state, remains for several months in a torpid condition, it affords a better opportunity of minutely following these changes, and of ascertaining in what manner they are effected, than most other insects; and the great comparative size of this species renders the investigation still more easy.

While in its larva state, this insect frequently changes its skin: it enlarges rapidly in size after each operation, and the nervous system undergoes a corresponding development. The author minutely describes the longitudinal series of ganglia, which extend the whole length of the animal. He remarks that the eleventh or terminal ganglion is distinctly bilobate, a form which, as suggested to him by Dr. Grant, is probably acquired by the consolidation of two ganglia which had been separate at an earlier period of development. A detailed account is then given of the nerves proceeding from these several ganglia.

During the change from the state of larva to that of the perfect insect, the number of the ganglia is found to diminish in consequence of the approximation and conjunction of adjacent ganglia; and the nervous cords which connect them are generally much shortened. A nerve is described which, from the mode of its distribution to the stomach, intestinal canal, and dorsal vessel, presents a remarkable analogy to the *par vagum*, or pneumogastric nerve of vertebrated animals; so that the author considers it probable that its functions are somewhat similar to this nerve; as has, indeed, been already conjectured by Straus-Dürckheim. Another division of nerves exist, which, from the principal branches derived from each abdominal plexus being always distributed among the tracheæ, near the spiracles, are perhaps analogous to the sympathetic system of nerves of the higher classes of animals.

When on the point of becoming a pupa, the nervous lobes above the œsophagus are found to be considerably enlarged, and to have assumed more of the appearance of a cerebral mass; while, at the same time, the nervous cords descending from them are shortened and thickened. The ganglia are brought nearer together, and their intervening cords lie between them in an irregular manner, the ganglia themselves being retained in their proper places in the segments by the nerves running transversely from them. The nerves of the antennæ are enlarged, and the optic nerves are become

much thicker and shorter than before. There is a remarkable enlargement of the thoracic nerves, particularly of those sent to the wings; and those belonging to the posterior pair of legs are curiously convoluted within the thorax, preparatory to their being uncoiled at the instant of the change being made to the pupa state.

These changes are followed minutely through several stages of development. The author expects to be able to lay before the Society, in a subsequent paper, the results of his investigation of the remaining stages, and to offer some observations upon the manner in which these changes are effected.

June 21.—Papers were read, bearing the following titles:

1. "An Account of the magnetical Experiments made on the Western Coast of Africa in 1830 and 1831," by Commander Edward Belcher of H.M.S. Etna. Communicated by the Rev. George Fisher, M.A. F.R.S., through Captain Beaufort, R.N. F.R.S.

The object of the inquiry specified in this paper, and of which the results are given in a tabular form, was to determine the relative horizontal intensities of terrestrial magnetism on the different parts of the coast of Africa which the author has been lately employed in surveying. The experiments were made with four needles constructed by Dollond on the model of those of Professor Hansteen; and the permanence of their magnetism during the voyage was verified by a comparison of trials made in England before and since the voyage. Errors arising from local causes of irregularity were guarded against by varying the places of observation at each station, and taking mean results.

2. "On the Use of a substance called the *False Tongue* in Foals," by Professor Sewell, of the Royal Veterinary College. Communicated by Sir Charles Bell, F.R.S.

The substance called the *false tongue*, which is thrown out from the mouth of the foal, either at the period of birth, or shortly before it, and to which various whimsical uses and virtues have been assigned, is conceived by the author to be requisite in this animal for the action of sucking, in consequence of its not respiring through the mouth, but altogether through the nasal passages: an instinctive feeling prompting it to supply the loss of that substance by sucking the teat of the mother. Dr. Prout, who analysed a portion of this substance at the request of the author, finds it to be composed principally of coagulated albumen slightly modified. The author regards it as a secretion from the tongue of the foal.

3. "Journal of the Weather, kept at High Wycombe during the year 1831, with monthly Observations," by James G. Tatem, Esq. Communicated by William Allen, Esq. F.R.S.

4. "Physical and Geological Observations on the Lake of Oo near Bagneres de la Chou, in the year 1831," by M. Nerée Boubée, Professor of Geology at Paris. Communicated by P. M. Roget, M.D. Sec. R.S.

The author ascertained that the bottom of the lake, which is 230 French feet in depth, forms a level plane of great extent, and is covered with a stratum of mud composed of fine micaceous sand of a

blue colour. The temperature of the bottom of the lake was 7° of the centigrade scale, at the middle 9°, at the surface 11°; that of the air varying from 14° to 15°. There was no indication of any current on the surface. A cascade 954 feet in height falls into the lake, carrying down the detritus of the surrounding rocks.

5. "Observations on the anatomy and habits of Marine Testaceous Mollusca, illustrative of their mode of feeding," by Edward Osler, Esq. Communicated by L. W. Dillwyn, Esq. F.R.S.

The author observes that in studying the physiology of the Mollusca, more satisfactory results may generally be obtained by tracing the organization connected with each important function, through different families, than by complete dissections of individual species; and, by thus connecting the study of function with that of structure, the zoologist is led to more certain inferences relating to those habits, the knowledge of which the pelagic character of the animal, and the difficulty of direct observation, would otherwise have rendered unattainable. The present paper is devoted to the anatomical investigation of the organs by which the food is received into the bodies of certain Mollusca. The herbivorous Mollusca which the author has examined have three modes of feeding. Some, as the *Trochus crassus*, browse with opposite horizontal jaws: others, as the *Turbo littoreus*, rasp their food with an armed tongue stretched over an elastic and moveable support: while others again, as the *Patella vulgata*, gorge it entire. The author enters into a minute anatomical description of the organs of manducation and deglutition, and also of that part of the nervous system situated in the neighbourhood of these organs, in each of these respective Mollusca,—illustrated by numerous drawings. He gives in each case a particular account of the mode of dissection, with a view to direct succeeding observers to obtain a distinct view of the parts he describes, and to verify the conclusions he has himself obtained.

He next notices a considerable modification in the structure of these organs which is presented in the Chiton. In this animal he finds a pair of simple lateral jaws, rather membranous than cartilaginous. Another variety of structure adapted for gorging food is met with in the *Patella mammillaris*, where there is simply a very muscular mouth and pharynx, but neither cartilage, tongue, nor hard part of any kind.

The apparatus by which the *Buccinum Lapillus* drills through shells in order to obtain its food, and the process it employs for that purpose, are next investigated; and that of the *Buccinum undatum* is particularly examined with the same view, the structure of the latter being very fully displayed.

The author hopes to be enabled to pursue these inquiries with respect to other tribes of Mollusca at some future period.

6. "On the Mammary Glands of the *Ornithorhynchus paradoxus*," by Richard Owen, Esq. Communicated by J. H. Green, Esq. F.R.S.

The author premises a history of the different opinions that have been entertained with respect to the anatomy and economy of this singular animal, which was first described and figured by Dr. Shaw

in the year 1792. The name of Ornithorhynchus, which it at present bears, was given to it by Blumenbach; and some account of the structure of the head and beak was given in the Philosophical Transactions by Sir Everard Home in 1800; and in a subsequent paper he states his opinion that this animal differs considerably from the true mammalia in its mode of generation, an opinion which was adopted by Professor Geoffroy St. Hilaire, who accordingly placed it, together with the Echidna, in a separate order designated by the term *Monotrèmes.* He afterwards formed this group into a distinct class of animals, intermediate to mammalia, birds, and reptiles. Oken and De Blainville, on the other hand, condemned this separation; and maintained that the monotremata should be ranked among mammalia, and as being closely allied to the marsupialia; and hazarded the conjecture that they possessed mammary glands, which they expected would ere long be discovered. Professor Meckel has since described these glands as being largely developed in the female Ornithorhynchus. He considers this animal, however, in the mode of its generation, as making a still nearer approach to birds and reptiles, than the marsupial tribe. He was unable to inject these glands in consequence of the contracted state of the ducts arising from the action of the spirit in which the specimen was preserved, and from their being filled with a concrete matter. Geoffroy St. Hilaire, in a subsequent memoir, persists in denying that these bodies possess the characters of mammary glands; but regards them as a collection, not of acini, but of cæca, having only two excretory orifices, and presenting no trace of nipples.

The author of the present memoir, having examined with great care the specimens of the female Ornithorhynchus preserved in the Museum of the Royal College of Surgeons, found the structure to correspond very exactly with the account given by Meckel; and, moreover, succeeded in injecting the ducts of these glands with mercury. He further notices the differences of development occurring in five different specimens: the size of these glands having an obvious and direct relation to that of the ovaria and uteri. The gland itself is composed of from 150 to 200 elongated subcylindrical lobes, disposed in an oblong flattened mass, converging to a small oval areola in the abdominal integument, situated between three and four inches from the cloaca, and about one inch from the mesial line. It is situated on the interior of the panniculus carnosus, the fibres of which separate for the passage of the ducts to the areola; the orifices of these ducts are all of equal size, and occupy an oval space five lines in length by three in breadth; not elevated however in the slightest degree above the surrounding integument. An oily fluid may be expressed from the ducts by squeezing the gland.

A minute description is then given of the anatomical structure of the internal genito-urinary organs of the female Ornithorhynchus: from which it appears that if the animal be oviparous, its eggs must, from the narrow space through which they have to pass in order to get out of the pelvis, be smaller than those of a sparrow; and no provision appears to be made for the addition of albumen or of shell in the

structure of that part of the canal through which they afterwards descend previous to their expulsion from the body. The ova are enveloped in a tough fibrous membrane in which the traces of vascularity, at least after being preserved in spirits, are not perceptible; whilst in birds the ova are attached by narrow pedicles, and are covered by a thin and highly vascular membrane.

From the whole of this inquiry, the author concludes that these glands are not adapted to the performance of any constant office in the œconomy of the individual, but relate to a temporary function. Their total absence, or at least their rudimentary condition, in the male, of which the author could perceive some traces in one specimen which he examined, and the greater analogy of their structure to a lacteal apparatus than to that of ordinary odoriferous glands, when taken in conjunction with the correspondence of their development to that of the uterine system, induce him to believe that they are to be regarded as real mammæ. This view is confirmed by the fact, noticed by Mr. Allan Cunningham, that the young of this animal readily takes cow's milk, and may be kept alive by this kind of sustenance.

7. "A Physiological Inquiry into the Uses of the Thymus Gland," by John Tuson, Esq. Communicated by J. C. Carpue, Esq. F.R.S.

The author is of opinion that the thymus gland is intended for two purposes: the one to serve as a receptacle of blood for supplying the chasm in the circulation occasioned by the great quantity sent to the lungs as soon as the function of respiration commences: the other to serve as a receptacle of osseous matter preparatory to the extensive ossification which is carried on in the early periods of growth.

8. "An Investigation of the Powers of the simple Supporters of Combustion to destroy the virulence of Morbid Poisons, and of the poisonous Gases, with a view to ascertain the possibility of controuling the extension of contagious or epidemic Diseases," by Edward Browne, Esq. F.L.S. Communicated by J. H. Green, Esq. F.R.S.

The author, after giving an account of the diversity of opinions entertained with regard to the power of chlorine gas to destroy contagion, states that this gas exerts a similar disinfecting power on the virus of small pox, and mentions the result of some experiments he tried on gonorrheal matter, on which it appeared to effect a similar change. Various experiments are stated to have been made with iodine and with oxygen, indicating the same disinfecting agency in these substances. The author conceives that these effects are promoted by the heat communicated to the respired air in the lungs. He conceives that sea air possesses a disinfecting power, which he explains by supposing that it contains a portion of iodine. He conjectures, from analogy, that fluorine and bromine may have the same property.

9. "Considerations on the Laws of Life, in reference to the Origin of Disease," by Adair Crawford, M.D. Communicated by T. J. Pettigrew, Esq. F.R.S.

The scope of this paper is to show the insufficiency of all theories

which attempt to account for the phænomena of the living body, either in health or disease, by an exclusive reference either to the solids or to the fluids which enter into its composition; or to the influence of an abstract and unknown principle of life; or to that of physical or chemical agents; or to the functions of the nervous, or of the vascular systems. For the establishment of the sciences of physiology and pathology upon the most solid foundations, the author is of opinion that all the circumstances above mentioned should be duly taken into account, and allowed their respective and proportionate degree of influence.

10. "On the Water Barometer erected in the Hall of the Royal Society," by J. F. Daniell, Esq. F.R.S. Professor of Chemistry in King's College, London.

The author having long considered that a good series of observations with a water barometer would be of great value as throwing light upon the theory of atmospheric tides, of the horary and other periodic oscillations of the barometer, and of the tension of vapour at different temperatures, was desirous of learning whether any such series of observations had ever been made. But he could meet with none having any pretensions to accuracy; for neither those of Otto Guericke, in whose hands the water barometer was merely a philosophical toy, nor the cursory notices of the experiments of Mariotte upon this subject contained in the History of the French Academy of Sciences, can be considered as having any such claim. The difficulties which opposed the construction of a perfect instrument of this kind long appeared to be insurmountable; but the author at length proposed a plan for this purpose, which, having been approved of by the late Meteorological Committee of the Royal Society, was ordered by the President and Council to be carried into execution.

The author then enters fully into the details of the methods he employed for constructing the whole of the apparatus, and for placing it in its present situation in the centre of the winding staircase conducting to the apartments of the Royal Society. The tube was very skilfully made by Messrs. Pellatt and Co. at the Falcon Glass-house. It was 40 feet long, and one inch in diameter at its lower end; and so nearly cylindrical, throughout its whole extent, as to diminish only by two tenths of an inch at its upper end. A second tube of the same dimensions was also made as a provision in reserve against any accident happening to the first. These tubes were both securely lodged in a square case by means of proper supports. A small thermometer with a platina scale, was introduced into the upper end of the tube. An external collar of glass was united to that end by heating it. This was done with a view of giving it additional support, and of preventing it from slipping. This end of the tube was then drawn out into a fine tube ready for sealing with the blowpipe; and a small stopcock was fitted on to it. The cistern of the barometer was formed by a small copper steam boiler, 18 inches long, 11 wide, and 10 deep, capable of being closed by a cock, and having at the bottom a small receptacle for holding the lower end of the tube, so

as to allow of the water in the cistern being withdrawn, without disturbing that contained in the tube.

The boiler was set with brickwork, in a proper position, over a small fire-place. It was nearly filled with distilled water, which was made to boil thoroughly so as to free it from air; and the cock being then closed, the water was raised in the tube by the pressure of the steam collected in the upper part of the cistern. The tube, when filled, was hermetically closed at the top: a proper scale, constructed by Newman, was applied to it, great care being taken to determine its height and to ensure the accuracy of its adjustments, and the precision of its measurements, by an exact mode of reading; and also to provide proper corrections for temperature. The water in the cistern was protected from contact with the air by being covered with pure castor oil to the depth of half an inch. The mercurial barometer employed as a standard of comparison, was of a portable construction, and was provided with a platina guard.

An account is then given of some of the results of the observations made with this water barometer, arranged in several sets of tables. The great object was to obtain good and uninterrupted series of observations, taken, at least once a day, at a fixed hour. The registers given by the author, contain such observations, continued for nearly a year and a half, namely, from October 1830 to March 1832. Some curious results are afforded by these observations. In windy weather the column of water is found to be in perpetual motion, not unlike that from the breathing of an animal. Many considerable fluctuations in the pressure of the atmosphere are rendered sensible by the motions of an aqueous column, which would totally escape detection by the ordinary mercurial barometer. Mr. Hudson remarked in the course of his observations, that the rise and fall of the water-barometer precedes by one hour the similar motions of the mercurial one. The most striking result of the comparison between the two, is the very near coincidence of the elasticity of the aqueous vapour, as deduced from the experiments, with its amount, as determined from calculation, in a range of temperature from 58° to 74°. But a gradually increasing difference was at length perceptible, showing that gaseous matter had by some means insinuated itself into the tube. When this became no longer doubtful, the boiler was opened, and it was found that a portion of the liquid oil had escaped; and that the remainder had become covered with large flakes of a mucilaginous substance, by means of which it is probable that a communication had been established between the air and the water. The water had, however, retained its purity, and no indication was afforded of the metal having been anywhere acted upon. The author recommends that if these researches are prosecuted, the water should be covered with a stratum of oil of four or five inches in depth, which he has reason to think will form an effectual barrier to all atmospheric influence.

11. "Hourly Observations on the Barometer, with experimental investigations into the phænomena of its periodical oscillation," by

James Hudson, Assistant Secretary and Librarian to the Royal Society. Communicated by J. W. Lubbock, Esq. M.A., V.P. and Treas. R.S.

Mr. Lubbock having found, from his examination of the meteorological observations made daily at the Royal Society, that they afforded no satisfactory result as to the daily variation of the barometer in consequence of the too great length of the intervals between the times of observation, the author undertook the task of making a series of hourly observations for a period sufficiently extensive to furnish preliminary data for explaining the anomalies of the barometrical oscillations. The present paper contains these hourly observations, amounting to about 3000 in number, and made in the months of April, May, June, and July, 1831, and in those of January and February of 1832. The standard barometer of the Society has been observed for about 16 or 18 hours during the day, through a period of 75 days; and also at every hour, through the whole twenty-four hours, for 30 days: the water barometer every hour, day and night, for 15 days; and the mountain barometer also every hour, day and night, for the same period. The relative levels of the surfaces of the fluids in the cisterns of each of these barometers, were accurately determined by Mr. Bevan. The most striking results afforded by these observations are exhibited by means of linear representations in four drawings which accompany the paper. The respective variations from each general mean, being referred, according to a given scale, to the mean line, and their points of distance from it, at each successive hour, being connected together by straight lines, the barometrical and thermometrical changes being each referred to the same scale, exhibits the striking connexion that exists between them. The comparison of the simultaneous movements of the three barometers shows the general accordance of their mean variations; and the precession in time, by about an hour, of the mean motions of the water barometer over those of the standard barometer; and also the precession, by the same interval, of the mean changes of this latter instrument over those of the mountain barometer. The author concludes by announcing many objects he has in view in the investigations in which he is at present engaged.

12. "Note on the Tides in the Port of London," by J. W. Lubbock, Esq. M.A., V.P. and Treas. R.S.

The author gives a comparative view of the predicted times of high water deduced from Mr. Bulpit's tables, White's Ephemeris, and the British Almanac, with the observations at the London Docks, from data furnished to him by Mr. Stratford; and also a comparison, by Mr. Deacon, at the London and St. Katherine's Docks.

13. "Researches in Physical Astronomy," by the same.

In this Paper a method is given of developing the disturbing function, in which the coefficients of the inequalities corresponding to any given order, are expressed in terms of the coefficients of the inferior orders; so that, for example, the coefficients of the terms in the disturbing function, multiplied by the squares of the eccentricities, are given analytically by means of the coefficients of those independent of the eccentricities, and of those multiplied by their first powers. As

the theorems, to which this method gives rise, are of great simplicity, the author considers them as deserving attention.

The Society then adjourned over the Long Vacation, to the 15th of November.

ROYAL ASTRONOMICAL SOCIETY.

March 9.—The following communications were read:—

I. A Letter from Mr. Snow to the Secretary, dated Jan. 2, 1832:

"I have the pleasure to say that I observed the late occultations of 119 and 120 *Tauri*, and of *Regulus*.

"119 *Tauri* before its occultation was gradually approaching the moon's dark limb, but it did not disappear until it reached the bright part of the moon, and vanished quite instantaneously upon touching the summit of a long, irregular, lunar mountain, without suffering the smallest alteration in colour or light before its disappearance.

"120 *Tauri* was not quite so certainly observed, as it disappeared just *before* it reached the bright part of the moon, which I was in hopes it would not have done. At the time the occultation of 119 *Tauri* took place, the moon wanted about 5^h of coming to the meridian, and was so nearly full when on the meridian, that both limbs were observed over the wires of the transit-instrument, and gave a semidiameter agreeing very nearly with that set down in the *Nautical Almanack*: the moon's R.A. thus determined was $5^h\ 31^m\ 3^s{\cdot}54$. However, when the occultation took place, the quantity that the moon wanted of being full was too small to be estimated by the eye." (Telescope 42-inch refractor; power 120.)

This letter was accompanied by a printed extract from the *Bibliothèque Universelle of July* 1831, containing Baron Zach's observation of the immersions and emersions of *Jupiter's* satellites on June 1, 1831; and also a notice of an astronomical board established in China, which appears to be the same as the well-known *Tribunal of Mathematics*. The number of members at present is seven, of whom three are Europeans.

II. Observations of the comets of 1830 and 1831, by different observers; also various computations of the elements of the said comets. Collected by Baron Zach, and communicated by Mr. Snow.

These observations, which were made in April and May 1830, and from January to March 1831, consist of right ascensions and declinations, and come from the observatory at Greenwich, Sir James South at Kensington, MM. Gambart at Marseilles, Wartmann at Geneva, Gautier at Chougny, Valz at Nismes, Encke at Berlin, and Rumker at Hamburg. The elements are by MM. Rumker, Valz, and Peters of Copenhagen.

III. Emersion of *Aldebaran* on Feb. 10, 1832, by the Rev. M. Ward. N. latitude 52° 43′ 45″·18. W. longitude $8^m\ 46^s{\cdot}8$.

	h m s
Instantaneous emersion of Aldebaran	2 57 28·9
Aldebaran transited mid. wire of circle	4 26 13·3
West limb of ☽ ditto ditto	4 28 51·3
Daily gain of the clock	1·63

IV. Stars observed with the Moon at Blackheath, from August 1831 to January 1832, by Mr. Wrottesley. The observations were made with a five-foot transit.

V. Observations made at the East India Company's Observatory at St. Helena, by Mr. Johnson.

These consist, first, of observations of the moon and moon-culminating stars from January to August 1830; secondly, of observations of the solstices of December 1829, and of June and December 1830. The latitude of the observatory deduced from them is $15° 55' 23''\cdot65$; while from several of the Greenwich stars, observed alternately by direct vision and reflexion, it is $15° 55' 26''\cdot54$.

VI. On the Planetary Theory, by Mr. Lubbock.

The object of this paper is to point out some simplifications which may be obtained in developing the functions R and $r\left(\frac{d\,R}{d\,r}\right)$ by the use of the binomial theorem. Mr. Lubbock applies this method to the determination of that part of $r\left(\frac{d\,R}{d\,r}\right)$ which contains the first powers only of the eccentricities.

VII. On the Rotation of *Venus*, by the Rev. Mr. Hussey.

Mr. Hussey's object in this paper is to show that the time of rotation of *Venus* asserted by Bianchini, of 23 days and 8 hours, is a near approximation to the truth, in opposition to Cassini and Schroeter, who fixed the same, the former at $23^h\ 15^m$, the latter at $23^h\ 21^m$; and to Sir W. Herschel, who, though he declares the time of rotation to be doubtful, thinks it cannot be so much as 24 days. The observations of Bianchini are quoted at length, in his own words, by Mr. Hussey, who also enters minutely into the arguments used by the younger Cassini, in support of his father's observations. From a review of the whole argument, Mr. Hussey concludes from Cassini, Maraldi, and Herschel, not having been able with powerful instruments to distinguish the spots of *Venus*, that their latitudes were unfavourable for such observations; that the observations of Schroeter are not to be depended upon, as Sir W. Herschel was unable to verify the same, with a more powerful telescope; that Cassini's observations are in the same predicament, having been made with an inferior instrument, imperfectly mounted and without a micrometer, and not having been much relied on by the observer himself; that we are justified in placing confidence in the observations of Bianchini, from the favourable circumstances under which they were made, the minuteness with which they are detailed, from their correctness having been ascertained by several bystanders, from the superior nature of the instruments employed, from the measurements being micrometrical, and from the character of the observer. Annexed to this paper were several diagrams of the spots of *Venus*.

VIII. Observations on the Magnitudes of Stars. By Mr. Birt; communicated by Mr. Lubbock.

These observations were made between April 1830 and January 1831. In the notes subjoined to them, the author has pointed out various discrepancies between the magnitudes assigned to the same star by different observers, from all of which, in some cases, his own

determination differs. The principal instances are *Pollux*, γ and α *Cassiopeæ*, α, ε, and ζ *Cephei*. $\varkappa$ and ι *Ophiuchi*, β and ε *Aquilæ*, and $\varkappa$ and λ *Lyræ*.

Among the presents announced this evening was a repeating Theodolite, by T. Jones, of Charing Cross, with horizontal circle of 20 inches diameter, graduated on silver, reading off to seconds by 3 micrometer microscopes, attached to a frame concentric with the circle, and on the same axis; with 30-inch transit telescope, with levels and divided circle, as in the great Theodolite of the trigonometrical survey. This valuable instrument was presented by J. Fuller, Esq. Fellow of the Society.

ZOOLOGICAL SOCIETY.

Proceedings of the Committee of Science and Correspondence.

Jan. 24, 1832.—Specimens were exhibited of various *Mammalia* and *Birds*, collected in Nepâl by B. H. Hodgson, Esq. Corr. Memb. Z. S., British Resident at Katmandoo. The *Mammalia* included specimens of the following species:—A new species of *Felis*, L., characterized as *Felis Moormensis*; the *Chiru Antelope*, *Antilope Hodgsonii* Abel; Mr. Hodgson's account of which, read to the Committee on March 22nd, 1831, will be found in the Phil. Mag. and Annals, N.S. vol. ix. p. 453; an *Antelope* new to science, characterized by Mr. H. as the *Antilope bubalina*; and the wild *Dog* of Nepâl, respecting which Col. Sykes, who was present, stated his impression that it was identical with the *Canis Dukhunensis*, described by him on a former occasion, (as noticed in Phil. Mag. and Annals, N.S. vol. x. p. 305, though he declined pronouncing a decided opinion on the point. They were accompanied by coloured figures, and, except in the instance of the latter, by accounts, which were read, of the several animals, from the pen of Mr. Hodgson.

Among the *Birds* contained in Mr. Hodgson's collection were the following species: *Hæmatornis undulatus*, a species described in our report of the Committee's proceedings on Dec. 27, 1830, and figured in Mr. Gould's Century of Birds; *Myophonus Temminckii*, the difference between which species and the *Myophonus flavirostris* (*metallicus*, Temm.) had been pointed out before the Committee on Dec. 27, 1830, as stated Phil. Mag. and Annals, vol. ix. p. 294; N.S.—a specimen of *Zoothera monticola*, deviating in no respect from that already described, Phil. Mag. and Annals, N.S. vol. ix. p. 295, and figured by Mr. Gould;—*Buceros Nepalensis*, an interesting species of *Hornbill*, described by Mr. Hodgson in the Asiatic Researches, vol. xvii. p. 178, but which had never before been seen in Europe; *Phasianus leucomelanos*, Lath. (Ind. Orn. ii. 633), the difference between which and the *Phasianus albo-cristatus*, described by Mr. Vigors in the Phil. Mag. and Annals, N.S. vol. ix. p. 60, was pointed out by that gentleman;—a new species of *Pigeon*, characterized by Mr. Vigors as *Columba Hodgsonii*.

A specimen was exhibited of the *Birgus Latro*, Leach, which

had recently been presented to the Society by Mr. J. P. Vaughan; and Mr. Owen confirmed, from the observations of MM. Quoy and Gaimard, and those of Mr. Cuming, the curious statement made by Herbst, that this *Crab* climbs trees for the purpose of stealing cocoa-nuts.

Mr. Owen subsequently reported the morbid appearances observed on the *post mortem* examination of the *Mandrill, Cynocephalus Maimon*, which recently died at the Society's Gardens.

Feb. 14.—The *Monkey* described in Phil. Mag. and Annals, N.S. vol. x, p. 311, under the name of *Semnopithecus? albogularis*, having died, it was placed upon the table, and Col. Sykes stated that its more essential anatomical characters were those of the genus *Cercopithecus*, of which he now preferred to consider it a species. Mr. Owen then read some notes on the Anatomy of this animal, which does not, he stated, present any remarkable deviations from the ordinary structure of the *Cercopitheci*. A specimen was exhibited of a Lemuridous animal, recently presented to the Society by C. Telfair, Esq. Corr. Memb. Z. S. It was shown by Mr. Bennett to possess characters differing to so great an extent from those of the previously known genera of the family to which it belongs, as to require its separation from them as the type of a new group, to which he gave the name of *Propithecus*, characterizing the species as *Prop. Diadema*.

Col. Sykes took occasion to add the *Viverra Rasse*, Horsf., to his Catalogue of the *Mammalia* of Dukhun, the two specimens exhibited to the Committee, which he had hitherto regarded as varieties of the *Viv. Indica*, Geoff., having been pronounced by Dr. Horsfield to be the *Viv. Indica*, and *Viv. Rasse*. The habitat of the former is in the woods of the western Ghauts; the latter is found in the table-land eastward of the Ghauts. Dr. Horsfield furnished an account of the differences between the two animals, adding, that not having been acquainted with the *Viv. Indica* at the time when he wrote the account of the *Viv. Rasse* in his "Zoological Researches in Java," he now found it necessary to modify the specific character of the latter.

Mr. Owen subsequently read some notes on a malformation of the beak of *Psittacus Erithacus*, L.

At the request of the Chairman, Mr. William Daniell, R.A., exhibited numerous drawings of *Antelopes* made by his brother from living animals, in his different journeys in Africa. He also exhibited drawings of the male and female *fire-backed Pheasant* (*Phasianus ignitus*, Lath.), which had also been made by his brother, in the native places of these birds.

Feb. 28.—Specimens were exhibited of numerous *Mollusca* and *Conchifera* hitherto undescribed, which form part of the collection made by Mr. H. Cuming during a voyage undertaken in 1827, 1828, 1829, and 1830, for the purpose of obtaining subjects in natural history on the western coast of South America, its adjacent islands, and many of those which form the principal Archipelago of the South Pacific Ocean. The specimens exhibited on the present oc-

casion constituted the first portion of the collection, which extends in these classes to upwards of four hundred new species; the whole of which Mr. Cuming proposed to bring before the Committee from time to time, as the descriptions of them are completed. The intention of publishing coloured figures of all the new species was announced.

The following is a list of the new species brought, on this evening, under the notice of the Committee, accompanied by characters and descriptions by Mr. Broderip and Mr. G. B. Sowerby:

CHITON *Goodallii, Stokesii, subfuscus, Lyellii, luridus, limaciformis, Blainvillii, Elenensis, Swainsoni, crenulatus, setosus, Frembleii, scabriculus*, and *retusus;* PLACUNANOMIA (new genus) *Cumingii;* DENTALIUM *splendidum, tesseragonum, quadrangulare, perpusillum;* HELIX *monile;* CAROCOLLA *globosa* and *quadridentata;* BULINUS *Broderipii, Coturnix, Coquimbensis, granulosus, cactivorus, nitidus, translucens, guttatus, vittatus*, and *scalariformis*; PARTULA *hyalina;* ACHATINA *Dactylus;* CYCLOSTOMA *Cumingii, succineum*, and *minutissimum;* FASCIOLARIA *granosa;* and VOLUTA *Cumingii.*

A paper was read by Mr. Cox, in which he entered at some length into the consideration of atmospheric causes as influencing the health of exotic animals kept in confinement in this climate.

March 13.— Mr. Gray described the following new animals, brought from New Holland by Mr. Cunningham: — PSEUDOMYS (new genus) *australis;* DIPLODACTYLUS (new genus of *Geckos*) *vittatus*; TILIQUA *Cunninghami.* He also stated that the comparison of a young specimen of *Mus giganteus*, Hardw., with a specimen of *Mus setifer*, Horsf., presented to the British Museum by their respective describers, had enabled him to correct an opinion expressed by M. Temminck in the "Tableau Méthodique," appended to his "Monographies de Mammalogie," that the latter species is only the young of the former; and he detailed the differences between the two animals, observing also, that the comparative length of the hinder feet, and the relative distances of the tubercles of the sole from the end of the toes and from the heel, appear to furnish very good distinctive characters for the species of this difficult genus.

Mr. Gray further stated, that in examining a specimen of *Antipathes* sent to the British Museum by the Rev. R. T. Lowe from Madeira, and which he believed to be identical with the *Ant. dichotoma*, Pall., he had discovered the animals of this remarkable *Coral*, and thus ascertained (what had previously been only presumed from the close resemblance of their horny *axes*,) its near relation to the genus *Gorgonia.*

Mr. Owen read an Account of the Anatomy of the *Ariel Toucan, Ramphastos Ariel*, Vig.

The stuffed skin and skull of a *Rodent Quadruped*, brought from Chili by Mr. H. Cuming, were laid upon the table, and characterized by Mr. Bennett as forming a new genus, OCTODON, of which the species before the Committee was also characterized by Mr. Bennett as *Oct. Cumingii.*

March 27.—A Report from Devereux Fuller, the Head Keeper, was read. It was communicated to the Committee by the President.

It referred to the experiments on the feeding of carnivorous *Mammalia* recommended by the Committee on Dec. 13, 1831, (Phil. Mag. and Annals, N.S. vol. xi. pp. 140, 288.) and subsequently ordered by the Council to be tried. The animals subjected to the experiment were two *Leopards* and two *Hyænas*: the whole of them were males.

On Jan. 11. the *Leopards* were weighed. No. 1 weighed 91lbs.: it was fed in the usual manner with 4lbs. of beef daily in one meal given in the evening. No. 2 weighed 100½lbs.: it was supplied with 2lbs. of beef at eight o'clock in the morning, and with a like quantity at the same hour in the evening daily. On Feb. 16, (after an interval of five weeks,) they were again weighed. No. 1 had gained in weight 1lb.: No. 2 had diminished in weight ½lb. No alteration was observed in the latter animal as regarded his daily exercise; but he became more ferocious than he had previously been, and was particularly violent.

On Dec. 23 the *Hyænas* were weighed. No. 1 weighed 86lbs.: it was fed as usual with 3lbs. of beef daily at one meal in the evening. No. 2 weighed 93lbs.: it was supplied with the same quantity of beef daily, divided into two equal portions, one of which was given in the morning and the other in the evening. On Feb. 16, (after an interval of eight weeks,) they were again weighed; and No. 1 was found to have increased in weight 1lb., while No. 2 had diminished in weight 1lb. The latter animal was observed to take less exercise than he had previously been accustomed to, and slept more than usual: his temper was not affected, and he did not exhibit unusual signs of hunger.

During the continuance of the experiment all the animals were fasted one day in each week in common with the other carnivorous species kept in the Menagerie.

From these experiments it appears that carnivorous *Mammalia* fed with two meals daily, do not continue in equally good condition with those which have the same quantity of flesh daily in one meal only. It further appears that in one instance (that of the *Leopard*,) the temper changed for the worse, and thus animals of the genus *Felis* might become more dangerous in a Menagerie from the ferocity they would acquire under such treatment; and that in another instance the habits were altered as regarded exercise, a diminution of which, in confined animals, must be injurious to health. The inference deduced in the Report is consequently in favour of the continuance of the accustomed mode of feeding the purely carnivorous animals with one meal daily.

The Report further stated that an experiment had been tried at the same time on the feeding of two animals less completely carnivorous than the preceding. They were weighed on Jan. 11. No. 1, a *Paradoxure Gennet*, weighed 4½lbs.: it was fed as usual with bread and milk in the morning, and with meat in the evening. No. 2, a *spotted Gennet*, weighed 7lbs.: it was fed with equal portions of

bread and milk on the morning and evening of one day; and with equal portions of flesh on the morning and evening of the next day; the quantity of food at each meal being the same as usual. On Feb. 16, (after an interval of five weeks,) the animals were again weighed. No. 1 weighed as before, and was in perfect health. No. 2 had lost in weight 1lb.: it had been during the alteration in its feeding much duller than usual.

The result of this experiment is in favour of the continuance of the plan hitherto pursued of feeding partially carnivorous animals with each kind of food on each day, and not on alternate days.

The exhibition of the new species of *Mollusca* and *Conchifera* collected by Mr. Cuming, which had been commenced Feb. 28, was resumed. The several shells exhibited were accompanied, as on the former occasion, by characters and descriptions from the pens of Mr. Broderip and Mr. G. B. Sowerby.

The following new species were characterized, and are described in the Proceedings of the Committee: CANCELLARIA *pulchra*, *solida*, *tuberculosa*, *bullata*, *Mitriformis*, *goniostoma*, (an approximation, Mr. Sowerby observes, to the shell named *Delphinula trigonostoma* by Lamarck, which would be properly placed in the genus *Cancellaria* next to this species,) *tessellata*, *Clavatula*, *obesa*, *brevis*, (another of those interesting species which form as it were the passage from the typical *Cancellariæ* to the species which Lamarck has placed among the *Delphinulæ* under the name of *Delph. trigonostoma*,) *rigida*, *Cassidiformis*, *ovata*, *acuminata*, *buccinoides*, (distinguished from *Buccinum* only by the two folds on the *columella*,) *indentata*, *hæmastoma*, *chrysostoma*, *gemmulata*, *decussata*, and *Bulbulus*; SCALARIA *Diadema*, (a fluid secreted by the animal produces a bright purple dye,); CARDITA *Cuvieri*, *tumida*, and *varia*; CRASSATELLA *undulata* and *gibbosa*; AMPHIDESMA *pulchrum*; MARGINELLA *Cypræola* and *Frumentum*; CHITON *pusillus*, *Grayii*, *Chiloensis*, *roseus*, *dispar*, *rugulatus*, *Columbiensis*, *punctulatissimus*, *hirundiniformis*, *lævigatus*, and *articulatus*; CYCLOSTOMA *flavum*, and STILIFER (new genus) *astericola* and *subulatus*.

April 10.—A Report from Devereux Fuller, the Head Keeper, was read. It was communicated to the Committee by the President.

It stated that the period of gestation of the *Puma*, *Felis concolor*, L., had been ascertained to be 96 or 97 days, the female in the Society's Menagerie having admitted the male on Dec. 28, and brought forth on the night of April 2, two young. The ground-colour of these is of a paler fawn than that of either of the parents, and they are deeply spotted, as was noticed on the former occasion (Phil. Mag. and Annals, N.S., vol. xi. p. 139.). The eyelids of one of them were partially unclosed on April 9. The mother, whose temper was always mild, has since become remarkably gentle, purring when the keeper goes into her den, and allowing her young ones to be handled and carried about without appearing to be annoyed by such treatment. The young, on the contrary, were when first born extremely fierce, hissing and scratching with all their might; they have, however, since become better tempered, though they are still

spiteful. The manners of both the mother and the young are similar to those of the *domestic Cat* and her kittens, the former carrying the latter about from place to place in her mouth. For a day or two previously to her littering she pulled the straw in her inner den into pieces and thus formed a nest.

On the former occasion the period of gestation could not be determined, the female having admitted the male several times; the last of which was 97 days prior to her parturition; a month after this latter occurrence (her single young one having been born dead,) she admitted the male once only, and became pregnant with her present litter.

A Note was read from Mr. Henry Tripp, of Orchard Wyndham, Somersetshire, respecting the provision made by a male *Hawk*, after the destruction of its female, for the nourishment of their young. On the morning after the first night of her absence five small birds were found placed on the side of the nest. These having been taken away, nine others were found on the second morning; among them were a *Blackbird* and a *Thrush*. All of them were picked but not in the least broken. On the third night the male bird was caught in a gin set in the nest for that purpose. He had previously been so shy as to evade all attempts at shooting him, while the female, on the contrary, was got at so readily as to induce the keeper to destroy her, notwithstanding his wishes first to destroy her mate.

Specimens and drawings of numerous animals referrible to the genus *Paradoxurus* were laid upon the table; and Mr. Gray entered into a detailed account of the distinguishing characters of the group, which he prefaced by some observations on the family of *Viverridæ* in general, and concluded by the description of several new species. He observed that the family may be divided, independently of the characters furnished by the teeth, into three sections, distinguished by the baldness or hairiness of the soles of their hinder feet, and by concurrent differences in the structure of their odorous glands. The first of these is limited to the true *Civets*, the genus *Viverra*, in which the under part of the hind-feet is entirely covered with hair, except on the tips of the toes and the large tubercles at their base; and the pouch secreting the civet forms a deep cavity on each side near the *anus*. The species of this group are: 1, the *African Civet*, *Viverra Civetta*, L.;—2, the *Zibet* of Buffon, Hist. Nat. tom. ix. t. 34, *Viv. Zibetha*, L., which is the *Viv. undulata*, Gray, Spic. Zool. p. 9, t. 8;—3, the *spotted Civet*, *Viv. Tangalunga*, Gray, which is the *Viv. Zibetha* of M. F. Cuvier, Dr. Horsfield, and Sir Stamford Raffles, and is readily distinguished from the last-mentioned species by a continuous longitudinal band occupying the upper surface of the tail, the numerous irregular rings being separated only on its inferior half;—4, the *Gunda Civet*, *Viv. Rasse*, Horsf., *Viv. Gunda*, Ham. MSS., which Dr. Horsfield believes to be distinct from *Viv. Indica*, Geoffr.;—5, the *pale Civet*, *Viv. pallida*, Gray;—and 6, the *Delundung*, *Viv. Linsang*, Hardw., *Felis gracilis*, Horsf. Of these the last three have the slender form of the *Gennets*; and one, the

last, has been formed into a separate genus by Dr. Horsfield; the teeth however, according to the figure of that naturalist, agree exactly with those of the *Civets*, except in the deficiency of the last upper molar.

The second section is likewise limited to a single genus, *Genetta*, in which the soles of the hinder feet have a narrow bald line extending from the heel and bifurcating, so as to inclose a small triangular hairy pad near the toes, the basal tubercle of which, and the tips of the toes themselves, are bald. In this section also the anal pouches exist, and the animals belonging to it, as well as to the former, when in confinement, frequently retrovert their tails, in order to press out, by rubbing against any hard substance within their reach, the odorous secretion contained in the pouches. The species are: 1, the *Fossane, Viv. Fossa*, Erxl.;—2, the *Senegal Gennet, Viv. Senegalensis*, Fisch., from M. F. Cuvier's 'Mammifères Lithographiés';—3, the *feline Gennet, Viv. felina*, Thunb., which has certainly no affinity with the *Civette de Malacca* of Sonnerat, doubtfully referred to it by M. Fischer:—and 4, the *common Gennet, Viv. Genetta*, L.

In the third section, which includes two very distinct subdivisions, the entire sole is bald from the toes to the heel. One of the subdivisions has long, slender, and nearly free toes; anal pouches of greater or less depth; and hair of a peculiarly harsh character and grizzled appearance: this includes the genera *Herpestes* and *Ryzæna*, and probably also *Crossarchus* and *Atilax*; but as Mr. Gray had not seen the two latter, he could not speak confidently with respect to them. *Crossarchus* and *Ryzæna* differ in having one false molar tooth less than the other genera. The remaining subdivision has the toes short, and united by a membrane as far as the base of the claws; it has no anal pouch, but in place of that organ a bald secreting fold over the sheath of the *penis*; and its fur is rather rigid with a woolly undercoat. In most cases the tail has the faculty of rolling itself up spirally from the tip, from which circumstance M. F. Cuvier deduced the generic name of *Paradoxurus* applied by him to the animals of this subdivision. One species, the *Benturong* of Major Farquhar, has since been separated by M. Valenciennes under the generic name of *Ictides*.

The teeth of the genus *Paradoxurus* agree in number and structure with those of *Viverra, Genetta*, and *Herpestes*, but differ in the form of the cheek-tooth and tubercular molars, which in both jaws are shorter, broader, and more bluntly tubercular, indicating more frugivorous habits. In their examination, not only in this genus but in the whole order, it is necessary to observe the change that takes place both in their distribution and form on the shedding of the milk-teeth, which are widely different from those by which they are succeeded. In the young of *Paradoxurus* there are in the upper jaw only four molars on each side, viz. two false molars, one cheek-tooth, and one tubercular; while the adult animal has one additional false molar, and a second tubercular, the third false molar taking the place of the cheek-tooth, and the cheek-tooth that occupied by the

tubercular, of the young animal. The teeth of the adult are also much stronger and larger, the anterior ones becoming less, and the posterior more, lobed and tubercular. In the first set, the false molars are thin and compressed, and the second is distinctly three-lobed; this last is replaced by a strong thick conical tooth with a slight raised margin behind, and the third or new false molar is nearly similar, but furnished with a very small tubercle in the middle of the inner side of the base of its crown. The cheek-tooth of the first set is also compressed and has a small lobe in the middle of the inner side; while in the second set this tooth is triangular, broad in front and narrow behind, with a large distinct lobe on the front of its inner margin. It is much larger than the tubercular tooth of the first set which it replaces, and which is little different in form from the first tubercular of the second set, although the latter is also larger and has more prominent and distinct tubercles.

Mr. Gray observed that it was on this discrepancy between the milk and second teeth that the generic character of *Paguma*, before described by him (see Phil. Mag. and Annals, N.S., vol. x. p. 234), was founded, he not having at that period noticed the change that takes place on the shedding of the former set. The description there given was taken from a skull belonging to a young animal about to part with its milk-teeth, which still however remained perfect, while the jaw had elongated sufficiently to allow of the partial development of the two tubercular teeth of the new set, which were rendered visible by scraping. In this state the true number of teeth belonging to the family was present, the tubercular tooth of the first set still retaining the place of the cheek-tooth of the second, for which it was described. Subsequently, however, Mr. Gray has been enabled, by cutting away the bone below this tooth, to lay bare the true cheek-tooth, which resembles that of the other species of *Paradoxurus*, to which genus the animal in question must therefore revert. The explanation of this change is the more interesting inasmuch as the Civets in general appear to attain nearly their full size previous to its occurrence, and consequently do not offer the usual indications of immature age.

Mr. Gray then proceeded to enumerate the following species of the genus *Paradoxurus*, all of them, as far as their *habitat* has been ascertained, natives of India and the Indian Islands.

Paradoxurus Typus, *Pennantii* (new sp.), *Bondar*, *prehensilis*, *Musanga*, *dubius* (new sp.), *hermaphroditus*, *Pallasii* (new sp.), *Crossii* (new sp.), *leucopus*, *Hamiltonii* (new sp.), *larvatus*, (*Paguma larvata*, Gray, Proceed. Comm. Zool. Soc. i. p. 96; Phil. Mag. and Annals, N.S. vol. x. p. 235.) *trivirgatus* (new sp.), and *binotatus*.

To this enumeration Mr. Gray added the indication of an animal known only by a rough sketch brought by Mr. Finlayson from Siam, and deposited in the Library of the East India Company. This he proposed to call *Parodoxurus Finlaysonii*, and described as being pale brown; with a band across the middle of the muzzle, and another across the orbits (including the eyes and expanding on the back of

the cheek), the ears, and three continuous narrow lines along the middle of the back, blackish brown; the feet blackish, and the tail cylindrical. He also considered it probable that the *Civette de Malacca* of Sonnerat, *Voy.* t. 91, the *Viverra Malaccensis* of Gmelin, belonged to this genus, with which it agreed in several particulars of its mode of colouring, although it differed in having a black streak along the middle line of its belly, a character confined to few among the *Mammalia.* With respect to the *Paradoxurus aureus* of M. F. Cuvier, he stated that he was inclined to believe that it really belonged to the genus on account of its naked soles, but was certainly not, as had been imagined, the young of *Par. Typus.*

Preparations were exhibited of the stomach and *cæcum* of a *Capromys* which had recently died at the Society's Gardens, and Mr. Owen read his Notes of the dissection of the animal, which are given in detail in the Proceedings of the Committee. He commenced by remarking that its external characters agreed with those described by M. Desmarest as existing in his *Capromys Fournieri;* while its admeasurements, especially those taken from the osseous system, corresponded closely with those given by Mr. Say in the Journal of the Academy of Natural Sciences of Philadelphia, when describing his *Isodon pilorides*, the species on which the generic characters were first pointed out. He further observed that the affinity of this genus to *Cavia*, indicated by Mr. Say from the comparison of *crania*, received corroboration from various particulars of the anatomy of the animal; an affinity, he conceived, not to be denied on account of the existence in *Capromys* of perfect clavicles, and their absence in *Cavia;* for an anatomical character, he observed, is not the less artificial if taken without reference to the rest of the organization. The individual examined was a fully grown male, and measured 1 foot 6 inches from the end of the nose to the setting on of the tail, the length of the tail being $7\frac{1}{2}$ inches.

April 24.—Lieut. Col. Sykes, having brought before the Committee, at previous meetings, various birds of the *Raptorial* and *Insessorial* orders, collected by him during his residence in Dukhun, completed on the present evening the exhibition of his collection of those orders.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

March 19.—A paper was read "On the phænomena of Newton's rings formed between substances of different refractive powers," by Professor Airy. In a previous communication to the Society, the author had announced as a result of theory not yet verified by observation, that "if a lens of a low-refracting substance were laid on a plate of a high-refracting substance, or *vice versâ,* and if the incident light were polarized in the plane perpendicular to the plane of reflection,—then, when the angle of incidence was less than the smaller polarizing angle, Newton's rings would be seen with centre black: when the angle of incidence was equal to that angle, the rings would disappear; when it was between the two polarizing

angles, the rings would be seen with centre white: when equal to the other polarizing angle, the rings would disappear; and beyond this the rings would again be seen with centre black." The paper now read, contained an account of experiments made with a lens of glass placed upon a plate of diamond. A theoretical calculation was given to show that the rings, when the angle of incidence was between the two polarizing angles, must be extremely faint; and that the combination of a plate of tourmaline with a doubly-refracting prism would be necessary to exhibit them in sufficient purity. Other precautions would also be necessary for the destruction of the light reflected at the upper surface of the lens, &c. The author then described in detail the appearances which he observed. While the angle of incidence was less than the polarizing angle of the glass, the centre of the rings was black; when equal to that angle, the rings disappeared: beyond it their centre was white; and beyond the maximum polarizing angle of the diamond their centre was black. The author considered that the agreement of these results with the theoretical anticipations afforded strong evidence of the general correctness of Fresnel's theory of reflection, and its perfect accuracy (as far as the senses can judge) with regard to reflection from glass. But in one respect there was a very curious deviation from theory. When the angle of incidence approached the maximum polarizing angle of the diamond, the rings, though very faint, did not disappear; but the white-centred rings were changed into black-centred rings by the contraction of all the rings, the first black ring contracting so far that the white spot disappeared, and the black ring became the central spot. This showed that on increasing the angle of incidence by a few degrees, the plane of undulation in the plane of reflection was retarded by nearly 180°. From this and other phænomena the author concluded, that the nature of reflection from the surface of diamond is different from that of any other reflection; that up to a certain angle it most resembles reflection from metals: through a small angle it has the peculiar property described above; and beyond this it does not sensibly differ from reflection at the surface of glass. These conclusions, he observed, do not coincide with those of Sir David Brewster.

LIX. *Intelligence and Miscellaneous Articles.*

BIELA'S COMET.

THIS comet, whose return was predicted in the present year and in the present month, has created almost as much alarm in the country as the dreadful ravages of the cholera, in consequence of the publication of some wild speculations relative to the probability of its encountering the earth in its progress, and involving us in one general ruin. The eyes of all the astronomers in Europe have been, for this month past, directed towards that quarter of the heavens, in which it was expected to be first visible, but without success,—if we except

the slight announcement in the public papers, that it had been seen at Slough, probably with one of Sir John Herschel's powerful telescopes. We have not heard, however, of any other person having been able to detect it. By an extract from a recent New York paper, it would appear that it has been seen in America; but we much doubt whether there exists in the whole of the United States a telescope powerful enough to detect the faint light which this comet is known to exhibit.

ON PARAFFIN AND EUPION.

Dr. Reichenbach has discovered two substances by the dry distillation of organic bodies, to which he has given the above names. The first from *parum affinis*, on account of its remarkable indifference or want of affinity; and the second from πιῶν or πιὸν fat, and ευ. These substances appear to be both contained in the tar of animal and vegetable substances. Beech-wood tar yields the most paraffin, and with the greatest facility; while the oil of Dippel gives most eupion.

If the tar obtained by the carbonization of beech-wood be subjected to distillation, the receiver, provided it has not been changed nor removed, contains three different liquids: at the top, light oil of tar, in the middle a watery acid liquor, and at the bottom heavy oil of tar. This last is to be subjected to repeated distillation; and when the product becomes rather thicker, and contains small shining particles, the receiver is to be changed, and the heat is to be increased as much as the glass will allow of, and until the residue becomes black and thick. The receiver then contains a yellow thick vapour, and an oily liquor, in which brilliant particles of paraffin are observable by transmitted light. If the liquor has not acquired the proper state, it is to be obtained by repeated distillations, and the paraffin may be separated in two different modes.

The first consists in mixing and shaking the distilled liquor with alcohol of specific gravity 0·837. After standing a little time, there deposits from the turbid mixture, a viscid liquid mass, which is to be repeatedly washed with alcohol of the same strength, until it is converted into small colourless plates. These are then to be dissolved in hot absolute alcohol, and as the solution cools, paraffin separates in small white needles, and in small plates: in order to purify them perfectly, they may be redissolved in hot absolute alcohol, from which they separate on cooling.

The following is a better method: Distil the heavy oil of tar repeatedly, and mix it gradually with one tenth of its weight of concentrated sulphuric acid, adding this quantity repeatedly until the mixture has become entirely black and fluid; this action is attended with heat and the evolution of sulphurous acid; the oil requires from a quarter to a half its weight of acid. If the heat does not rise to 212° Fahr. it must be raised to that degree artificially. The mixture is then to remain at least twelve hours exposed to a heat of not less than 124° Fahr., in order that the paraffin may not congeal; it then is found as a colourless liquid on the surface. Decant this liquid, which is a compound of paraffin and a peculiar oil; or when all is cold, let it be taken off in a cake, break it, wash it with water, and

press it in bibulous paper. By this method the oil is absorbed by the paper, and the paraffin remains in small scales, which are to be purified by solution in hot absolute alcohol: it may afterwards be melted into one mass, under hot water, and should then be colourless and transparent as glass, dry and slightly fusible, and make no greasy spot on bibulous paper.

Sometimes it happens that the combination of paraffin and oil does not separate properly from the sulphuric acid; in that case it is to be distilled; water, sulphuric acid, and an oil evaporate: as soon as the last thickens, it then contains paraffin, which is to be separated and treated as before, with sulphuric acid, alcohol, &c. If this compound is not quite colourless, it is to be allowed to congeal, and treated with concentrated sulphuric acid; then, in order to purify it, it is to remain long in a warm place.

The properties of paraffin are, that at common temperatures it is hard, crystalline, perfectly white, inodorous, tasteless, brittle, its touch like that of cetine, ductile, but not easily uniting, streak greasy, a non-conductor of electricity, loses no sensible weight during months of exposure to the air, melts at about 111° Fahr. into a colourless, transparent, oleaginous fluid, boils at a higher temperature, and afterwards evaporates in white vapour, suffers no change by distillation, and leaves no residue, becomes coloured only when combined with other organic substances. By the flame of a taper it fuses with out burning; when heated in a platina spoon until it begins to evaporate, it will inflame in the candle, and burns with a pure white flame without soot or residue. A match made with it, burns like a taper, without smell; bibulous paper rubbed on it does not absorb it; at common temperatures it has not a greasy feel. Its density is 0·870.

It has been already stated, that paraffin is so named on account of its indifference or slight affinity for other bodies. The following have not the least effect upon it: chlorine, whether in the state of gas or of solution; sulphuric, muriatic, nitric, acetic, oxalic, and tartaric acids; solutions of potash, ammonia, lime, barytes, strontian; the alkaline carbonates, hydrate of lime, potassium even in fusion; deutoxide of lead and peroxide of manganese. Sulphur, phosphorus, and selenium do not fuse with paraffin; when mixed with it after having been fused, it appears to take up only a very small quantity. It does not combine by fusion with camphor, naphthaline, benzoin, nor pitch, but unites well with stearine, cetine, bees' wax, and colophony. Lard and suet melt with it, but separate on cooling. Olive oil, when cold, dissolves paraffin imperfectly, but readily when hot; oil of almonds acts more slowly. The oils of turpentine and tar, and naphtha, dissolve it readily, even when cold; 100 parts of æther dissolve 140 parts of paraffin at 77° Fahr.; at a rather lower temperature it congeals into a white crystalline mass. Absolute alcohol dissolves but little when cold, and even this little is precipitated by water; alcohol when boiling dissolves only 3·45 per cent. of its weight, and the solution congeals on cooling. Test papers are not altered by the spirituous solution.

Paraffin appears applicable to several useful purposes. It gives

better light than wax, and improves spermaceti for candles; it may be extremely useful as a cement, because it is not acted upon either by acids or alkalies; it may also serve to grease carriage-wheels, &c.

Eupion is best prepared by the following process:—Put into an iron retort 14 pints (imperial) of fresh rough animal tar, prepared from flesh, bones, hoofs or horns, and draw off $8\frac{3}{4}$ pints; redistil and draw off only $5\frac{1}{4}$ pints; shake it carefully, and by small portions, with 18 ounces (avoirdupois) of sulphuric acid. By this there are obtained a red solution, and a subtile transparent liquid of a bright yellow colour; the latter being separated, is to be mixed in a retort with an equal weight of sulphuric acid, and three fourths are to be distilled. The colourless product is to be washed with a solution of potash, and after being some time digested, the oil is to be separated and again distilled with half its weight of sulphuric acid; distil again, wash with a hot solution of potash; decant, and then distil very slowly with pure water until three fourths pass into the receiver,—there then remains some paraffin still mixed with the eupion. The distilled eupion is to be put over sulphuric acid in the air-pump for twenty-four hours; it is then to be distilled with a few grains of potassium, which occasions it to deposit some brown flocks of a red brown colour, which are to be separated; when after repeating this treatment it is no longer rendered turbid, but leaves the potassium of a metallic whiteness, it is to be decanted; it is not pure unless it burns without smoke, and its density exceeds 0·740. The eupion is separated from the paraffin either by distillation with a large quantity of water, because it is rather more volatile than paraffin; or by spirits of wine, in which paraffin is insoluble; or by extreme cold, which makes it crystallize. The distillation with water, when only the first portions are received, renders it entirely free from paraffin. By the processes which have been described, and with slight modifications, eupion is obtained from vegetable tar, and paraffin from animal tar.

The properties of eupion are the following: Colourless, transparent as water, liquid even at 4° Fahr., tasteless, inodorous, unalterable in the air, is a non-conductor of electricity, has no effect upon litmus or turmeric papers, is as fluid as absolute alcohol, forms drops at 68° Fahr. 0·296 of the size of those of water, spreads very readily on glass, but rises in a glass tube only to 0·6207 of the height that water does, forms a spot upon bibulous paper, which disappears in time, but more readily when heated. Its density is 0·740; from 66° Fahr. to 336°, increases about one fifth, boils at 336°, and volatilizes if it be pure. It does not inflame in a cup by a taper, but readily when heated in a platina spoon, is readily fired by a match, with a bright flame without smoke, even when the flame is as long as the hand.

Eupion is perfectly insoluble both in cold water and hot; 100 parts of absolute alcohol at 65° Fahr. dissolve 33 parts; but on cooling, a great part of the eupion separates. These two fluids when hot mix in all proportions. Æther mixed with a tenth of eupion forms a clear solution, but with five times that quantity it is turbid; it becomes clear, however, on standing, during which water evaporates from the æther: acetic æther dissolves about one third of its weight of eupion: sulphuret of carbon, oil of turpentine, naphtha, oil of almonds and of

olives, readily mix with this fluid even when cold. Eupion, when cold, readily dissolves chlorine, and bromine still more so; but heat separates these bodies without altering it. Iodine dissolves in it even in the cold with its violet colour, and much more readily when hot, and on cooling it crystallizes in part. Phosphorus, selenium, and sulphur dissolve readily in eupion when heated, but not when cold; on cooling, the two former precipitate totally, and the latter partially. Naphthaline, camphor, stearine, cetine, cholesterine, paraffin, and balsam of copaiba dissolve in it in the cold, and much more so when hot. Tallow dissolves in it at 80° Fahr., but at 68° the solution becomes clotted, probably the stearine separates, and the elaine remains in solution. Bees' wax dissolves in eupion when heated, but the greater part separates on cooling. Colophony is partially soluble in the cold, but perfectly at a boiling heat. Benzoin, gum anime, copal, and gum lac, dissolve only partially at a boiling heat, and they precipitate either totally or partially on cooling. Caoutchouc swells in eupion in an extraordinary manner, yet does not dissolve in it in the heat of a stove, but readily at a boiling temperature. The solution does not dry by exposure to the air. Heated upon a plate of glass in a stove, it soon becomes adhesive, may be drawn into threads, and eventually dries. The caoutchouc remains as a brittle varnish, which may be scraped off in small scales like dried gum or varnish.

The following substances have no action upon eupion: Concentrated nitric acid, concentrated sulphuric acid, muriatic, acetic, oxalic, tartaric, succinic, and citric acids; potassium, hydrate of potash, hydrate of lime, solutions of potash, lime, barytes, strontian, and ammonia; the carbonated alkalies, deutoxide of lead, peroxide of mercury, peroxide of manganese, oxide of copper, bichromate of potash.

Eupion is an excellent substance for keeping potassium in, probably also for separating stearine from elaine; and is a most remarkable substance for giving light by combustion, giving no soot even when mixed with paraffin.—*Ann. de Chim. et de Phys.* l. p. 69.

LUNAR OCCULTATIONS FOR NOVEMBER.

Occultations of Planets and fixed Stars by the Moon, in November 1832. Computed for Greenwich, by THOMAS HENDERSON, *Esq.; and circulated by the Astronomical Society.*

1832.	Stars' Names.	Magnitude.	Ast. Soc. Cat. No.	Immersions.				Emersions.			
				Sidereal time.	Mean solar time.	Angle from North Pole.	Angle from Vertex.	Sidereal time.	Mean solar time.	Angle from North Pole.	Angle from Vertex.
				h m	h m	°	°	h m	h m	°	°
Nov. 7	μ Ceti	4	293	23 36	8 27	136	104	0 41	9 32	276	253
10	χ^1 Orionis .	5	729	7 57	16 36	73	106	9 1	17 40	293	331
13	o Cancri	4·5	1066	1 27	9 55	55	19	2 12	10 40	297	257
15	53 l Leonis.	6	1284	6 27	14 46	66	28	7 32	15 51	258	224

AN EPHEMERIS OF THE STARS PROPER TO BE OBSERVED WITH *MARS*, AT THE ENSUING OPPOSITION OF THAT PLANET.

[Continued from p. 326.]

1832.	Stars.	Mag.	Apparent Place.		Semidiameter.		Hor. Par.
			Right Ascens.	Declin. *North.*	In time.	In arc.	
			h m s	° ′ ″	s	″	″
Nov. 8	A^1 Tauri	5	3 54 49,39	21 37 1,7			
	Mars^2	S	4 1 43,65	21 2 36,7	0·675	9,46	16,90
	53 Tauri	6·7	9 35,27	20 43 49,3			
9	A^1 Tauri	5	3 54 49,41	21 37 1,7			
	Mars^2	N	4 0 14,61	21 1 38,9	·676	9,47	16,92
	53 Tauri	6·7	9 35,29	20 43 49,3			
10	A^1 Tauri	5	3 54 49,43	21 37 1,8			
	Mars^2	S	58 43,88	21 0 33,2	·676	9,48	16,93
	53 Tauri	6·7	4 9 35,30	20 43 49,3			
11	* Tauri (*b*)	8	3 47 23,56	20 49 51,1			
	Mars^2	N	57 11,67	20 59 19,7	·677	9,48	16,94
	53 Tauri	6·7	4 9 35,32	20 43 49,4			
12	* Tauri (*b*)	8	3 47 23,57	20 49 51,1			
	Mars^2	S	55 38,15	20 57 58,5	·677	9,48	16,95
	53 Tauri	6·7	4 9 35,34	20 43 49,4			
13	* Tauri (*b*)	8	3 47 23,59	20 49 51,1			
	Mars^2	N	54 3,55	20 56 30,0	·677	9,47	16,94
	53 Tauri	6·7	4 9 35,35	20 43 49,4			
14	* Tauri (*b*)	8	3 47 23,60	20 49 51,2			
	Mars^2	S	52 28,07	20 54 24,2	·676	9,46	16,94
	53 Tauri	6·7	4 9 35,37	20 43 49,4			
15	* Tauri (*b*)	8	3 47 23,61	20 49 51,2			
	Mars^2	N	50 51,93	20 53 11,6	·675	9,45	16,92
	53 Tauri	6·7	4 9 35,38	20 43 49,5			
16	* Tauri (*b*)	8	3 47 23,62	20 49 51,2			
	Mars^2	S	49 15,35	20 51 22,6	·674	9,44	16,90
	A^1 Tauri	5	54 49,51	21 37 2,0			
17	* Tauri (*b*)	8	3 47 23,64	20 49 51,3			
	Mars^2	N	47 38,55	20 49 27,7	·673	9,42	16,87
	A^1 Tauri	5	54 49,52	21 37 2,0			
18	Mars^2	S	3 46 1,77	20 47 27,4	·671	9,40	16,83
	* Tauri (*b*)	8	47 23,65	20 49 51,3			
	A^1 ——	5	54 49,54	21 37 2,1			
19	Mars^2	N	3 44 25,23	20 45 22,0	·669	9,38	16,79
	* Tauri (*b*)	8	47 23,66	20 49 51,3			
	A^1 ——	5	54 49,55	21 37 2,1			
20	Mars^2	S	3 42 49,15	20 43 12,0	·667	9,35	16,74
	* Tauri (*b*)	8	47 23,67	20 49 51,4			
	A^1 ——	5	54 49,56	21 37 2,1			
21	Mars^1	N	3 41 13,76	20 40 58,1	·664	9,32	16,69
	* Tauri (*b*)	8	47 23,68	20 49 51,4			
	A^1 ——	5	54 49,57	21 37 2,2			

1832.	Stars.	Mag.	Apparent Place. Right Ascens.	Apparent Place. Declin. *North.*	Semidiameter. In time.	Semidiameter. In arc.	Hor. Par.
			h m s	° ′ ″	s	″	″
Nov. 22	Mars[1]	S	3 39 39,27	20 38 40,9	0·662	9,29	16,63
	* Tauri (*b*)	8	47 23,69	20 49 51,4			
	A[1] ——	5	54 49,58	21 37 2,2			
23	F[1] Tauri	6·7	3 32 41,40	19 9 31,9			
	Mars[1]	N	38 5,93	20 36 21,0	·659	9,26	16,56
	32 Tauri	6	47 0,26	21 59 25,0			
24	F[1] Tauri	6·7	3 32 41,41	19 9 31,9			
	Mars[1]	S	36 33,94	20 33 59,3	·656	9,22	16,49
	32 Tauri	6	47 0,26	21 59 25,0			
25	* Tauri (*a*)	9	3 29 18,97	20 21 54,1			
	Mars[1]	N	35 3,49	20 31 36,4	·653	9,18	16,42
	32 Tauri	6	47 0,27	21 59 25,0			
26	* Tauri (*a*)	9	3 29 18,98	20 21 54,1			
	Mars[1]	S	33 34,80	20 29 12,8	·649	9,13	16,33
	32 Tauri	6	47 0,28	21 59 25,1			
27	* Tauri (*a*)	9	3 29 18,99	20 21 54,1			
	Mars[1]	N	32 8,03	20 26 49,4	·646	9,09	16,24
	32 Tauri	6	47 0,28	21 59 25,1			
28	* Tauri (*a*)	9	3 29 18,99	20 21 54,2			
	Mars[1]	S	30 43,35	20 24 26,9	·642	9,04	16,15
	32 Tauri	6	47 0,29	21 59 25,1			
29	65 Arietis	6	3 14 48,28	20 12 19,6			
	* Tauri (*a*)	9	29 19,00	20 21 54,2			
	Mars[1]	N	29 20,94	20 22 6,0	·638	8,98	16,05
30	65 Arietis	6	3 14 48,29	20 12 19.6			
	Mars[1]	S	28 0,93	20 19 47,3	·634	8,92	15,95
	* Tauri (*a*)	9	29 19,00	20 21 54,2			
Dec. 1	65 Arietis	6	3 14 48,29	20 12 19,6			
	Mars[1]	N	26 43,48	20 17 31,4	·630	8,86	15,85
	* Tauri (*a*)	9	29 19,01	20 21 54,2			
2	65 Arietis	6	3 14 48,29	20 12 19,6			
	Mars[1]	S	25 28,68	20 15 19,1	·625	8·80	15.74
	* Tauri (*a*)	9	29 19,01	20 21 54,2			
3	65 Arietis	6	3 14 48,30	20 12 19,6			
	Mars[1]	N	24 16.67	20 13 10,8	·621	8,73	15,63
	* Tauri (*a*)	9	29 19,02	20 21 54,3			
4	65 Arietis	6	3 14 48,30	20 12 19,7			
	Mars[1]	S	23 7,55	20 11 7,1	·616	8,66	15,51
5	65 Arietis	6	3 14 48,30	20 12 19,7			
	Mars[1]	N	22 1,42	20 9 8 6	·611	8,60	15,39
	F[1] Tauri	6·7	32 41,47	19 9 32,1			
6	65 Arietis	6	3 14 48,30	20 12 19.7			
	Mars[1]	S	20 58,33	20 7 15,7	·606	8,53	15,27
	F[1] Tauri	6·7	32 41,47	19 9 32,1			

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Giddy at Penzance, and Mr. Veall at Boston.

Days of Month, 1832.	Barometer.					Thermometer.					Wind.			Rain.		
	London.		Penzance.		Boston	London.		Penzance.		Bost.	Lond.	Penz.	Bost.	Lond.	Penz.	Bost.
	Max.	Min.	Max.	Min.	8½ A.M.	Max.	Min.	Max.	Min.	8½ A.M.						
Sept. 1	29·748	29·638	29·654	29·610	29·10	70	47	61	50	56	W.	NW.	calm	0·20	0·085	...
☽ 2	30·113	29·947	30·104	29·910	29·30	69	43	65	49	56·5	W.	NW.	calm	...	...	0·05
3	30·245	30·191	30·160	30·148	29·60	72	43	64	50	56·5	W.	NE.	calm	...	...	...
4	30·286	30·153	30·154	30·098	29·68	72	45	64	48	55	NE.	NE.	calm	...	...	...
5	30·098	30·066	29·954	29·898	29·52	72	46	63	54	55·5	W.	NE.	calm	...	...	...
6	29·998	29·880	29·904	29·748	29·41	67	52	64	53	56	E.	SE.	calm	·61	...	...
7	29·957	29·826	29·898	29·724	29·25	65	48	65	58	58·5	W.	NW.	calm	·01	...	...
8	30·043	30·035	29·954	29·951	29·40	72	48	64	54	60	W.	NW.	NW.	...	...	...
9	30·010	29·915	29·957	29·754	29·27	69	54	65	54	60	W.	W.	SW.	·01	·310	...
○ 10	29·909	29·768	29·887	29·760	29·11	68	47	62	55	57	W.	NW.	NW.	·14	...	·16
11	30·279	30·071	30·207	30·060	29·32	67	47	61	53	58	W.	NW.	NW.	...	·030	...
12	30·322	30·237	30·213	30·204	29·58	54	49	64	52	58	W.	NW.	W.	...	...	...
13	30·073	29·898	30·057	30·004	29·32	62	46	62	55	59	W.	NW.	W.	·04	·320	·49
14	29·899	29·881	30·013	29·990	29·16	64	49	60	52	52·5	W.	NW.	NW.	...	...	...
15	30·169	29·928	30·010	29·990	29·38	64	42	62	54	53	NW.	NW.	NW.	...	...	...
16	30·291	30·234	30·260	30·213	29·60	64	52	62	52	57	NW.	NW.	W.	...	...	...
☾ 17	30·254	30·160	30·260	30·160	29·62	68	53	63	55	60	W.	W.	W.	...	...	...
18	30·189	29·984	30·160	29·963	29·30	65	37	60	54	58	W.	NE.	W.	...	...	...
19	30·477	30·353	30·366	30·266	29·71	60	34	59	49	48	N.	NE.	N.	...	...	...
20	30·546	30·540	30 416	30·396	29·88	67	39	58	47	54	NW.	NE.	NW.	...	...	...
21	30·557	30·536	30·436	30·410	29·93	66	52	61	47	54·5	N.	SE.	NW.	...	...	...
22	30·515	30·410	30·210	30·204	29·94	66	46	62	51	58	E.	SE.	calm	...	...	...
23	30·383	30·268	30·254	30·204	29·75	77	38	63	54	56	E.	S.	W.	...	...	...
● 24	30·403	30·376	30·254	30·248	29·70	81	42	64	50	55	S.	S.	W.	...	...	...
25	30·427	30·379	30·278	30·274	29·70	82	44	64	54	55	W.	SW.	calm	...	...	...
26	30·314	30 212	30·204	30·178	29·48	79	44	64	54	60	SW	NW.	calm	...	...	...
27	30·181	30·086	30·104	30·098	29·55	79	41	64	52	56	S.	NW.	calm	...	...	...
28	30·060	30·048	29·984	29·948	29 44	78	43	64	54	59	S.	NW.	calm	...	...	...
29	30·044	29·781	29·904	29·774	29·45	79	52	61	55	58·5	S.	SW.	calm	·09	...	·20
30	29·943	29·929	29·757	29·754	29·33	66	47	63	57	61	SW.	SW.	W.	·02	0·240	...
	30·557	29 638	30·436	29·610	29·49	82	31	65	47	56·7				1·12	0·985	0 90

Remarks.

London — September 1. Fine: showery. 2, 3. Fine. 4. Hazy: clear. 5. Slight fog: very fine. 6. Fine: heavy rain at night. 7. Fine: rain: fine at night. 8. Very fine. 9. Overcast: fine. 10. Heavy rain: fine. 11. Very fine. 12. Foggy: cloudy: fine at night. 13. Rain. 14. Clear: windy. 15. Cloudy: fine. 16, 17. Very fine. 18. Slight haze. 19, 20. Clear and fine. 21. Foggy in the morning: fine. 23, 24. Very fine. 25—29. Foggy in the mornings: very hot through the days. 30. Showery.

Penzance.—Sept. 1. Fair: showers. 2. Clear: fair. 3, 4. Clear. 5—7. Fair. 8. Clear: fair. 9. Fair: rain at night. 10. Fair. 11. Showers: fair. 12. Fair. 13. Rain: fair. 14—17. Fair. 18. Fair: misty. 19. Fair. 20. Fair: clear. 21. Clear. 22. Fair. 23. Fair: clear. 24—27. Clear. 28. Fair. 29. Rain: fair. 30 Fair: rain.

Boston.—Sept. 1. Cloudy: rain P.M. 2—4. Fine. 5—7. Cloudy. 8. Fine. 9. Cloudy. 10. Cloudy: rain early A.M. 11, 12. Fine. 13. Fine: rain early A.M. 14, 15. Fine. 16. Cloudy. 17—19. Fine. 20. Cloudy. 21. Fine. 22. Cloudy. 23—26. Fine. 27. Foggy. 28 Fine. 29. Cloudy: rain early A.M. 30. Cloudy.

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[THIRD SERIES.]

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LXX. *On the Variations which Temperature produces in the Double Refraction of Crystals.* By FREDERICK RUDBERG, *Professor of Physics in the University of Upsal*.*

THE researches of M. Mitscherlich having demonstrated that the angles of crystals which do not belong to the regular system, change their magnitude with the temperature, and that the dilatation is consequently different, according to the principal directions of these bodies, or according to their axes of crystallization, there was reason to believe that the double refraction also would vary with the temperature. The existence of this variation was afterwards established by ulterior researches, which M. Mitscherlich, in a manner as simple as it was ingenious, made by the method of interferences, by observing the compensation effected by crossing plates of crystals at different temperatures. By this method, however, we obtain only the ratio between the mean double refraction of the crystal in a cold and in a heated state, without being able to determine how much the refraction of each of the two rays into which the light divides itself, has separately varied with the difference of temperature. In order to decide this question, we must obviously determine directly the refraction at a high temperature; and the following are the results of such an inquiry, made with *rock crystal*, *calcareous spar*, and *arragonite*.

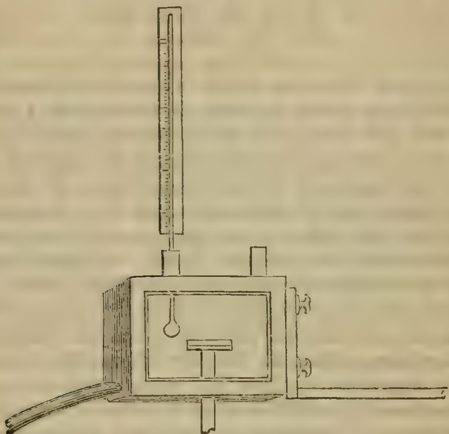
The experiments were made in the same place, and by means of the same instruments and the same prisms, as my former experiments† on the refraction of the same minerals at the temperature of the air; and for this reason I do not

* Communicated by the Author.

† See our present volume, p. 1, and 136.—EDIT.

here compare them with these. In order to maintain a constant temperature during the whole time that an experiment lasted, it was necessary to have a particular apparatus, of which I shall first give a description.

A box, of the form of a parallelopiped, was made of white iron, and so strong that four of its faces were double, and formed with each other a shut-up space, which communicated only at one side, and beneath one surface, with a small steam boiler; and at the other side, above a second surface, with the external air. The other two surfaces were formed with plates of mica, so that there was an inclosed space which contained only air, and which, when the prism was placed in it, was heated by the steam, which, surrounding the four



sides, circulated only in the space between the four double surfaces, without being permitted to mix with the interior air. The temperature of this space was indicated by a thermometer put into a cork, which was introduced into a tube passing through the two upper surfaces of the box, and which completely closed the tube, in order to prevent the heated air from escaping upwards. A similar tube passing through the two lower surfaces of the box formed a free communication between the interior and the exterior air, so that their elasticity remained always the same. In the middle of this tube, without touching its sides, there rose from the centre of the repeating circle a vertical copper rod, carrying on its summit a plate, upon which the crystal was placed. This rod was attached below to another plate of copper, which, instead of

the plate of ground-glass, upon which the prism was placed in my former experiments, rested on the ring of copper, which having teeth upon its circumference could be put in motion by a screw. By this arrangement, which allowed me to turn the prism, it was as easy as formerly, when the prism remained in the open air, to perform all the operations necessary for the exact determination of the refraction, without being obstructed by the heating apparatus, which being on one side united with the boiler by a tube, was on the other side attached to a rod of iron, rising from the masonry on which the repeating circle rested.

The temperature of the interior of the box remained, by this means, perfectly invariable during the time occupied by each experiment. From the temperature, however, of the external air, of which the extremes were on different days $+12°$ and $+20°$, it varied from day to day between $+76°$ and $+84°$; so that there was almost a constant difference of 64°.

With respect to the experiments themselves, I ought to remark, that no change in the dispersion could be observed, and that for this reason, I determined the ratio of refraction only for the single ray F* of the spectrum. At the beginning of each experiment the prism being at the temperature of the external air, and being turned till the ray F was at its minimum deviation, the variation in the deviation produced by heating was measured in this position of the prism. For crystals with one optic axis this determination was the only one to be made, because the edge of the prism being parallel to the axis of crystallization, the refracting angle did not change with the temperature. But for crystals with two optic axes, it was necessary, besides this, to determine the change which the difference of temperature produced in the refracting angle, which, as the following results will show, was very considerable.

With respect to the calculation of the index of refraction, it must be observed that the refracting power of the air surrounding the prism being diminished, the deviation becomes at first directly increased, but at the passage of the ray through the last plate of mica, it is, on the contrary, somewhat diminished. These two corrections must be determined separately. According to the experiments of MM. Biot and Arago, the index of refraction of air at 0°, and at a barometrical pressure $= h$, is

$$= \sqrt{1 + \frac{0{\cdot}000588767}{1{\cdot} + 0{\cdot}00375t} + \frac{h}{0^{m}{\cdot}76}}.$$

* This ray or dark line is nearly the boundary between the *green* and the *blue* space.—EDIT.

The elasticity of the internal and the external air being the same, and almost equal to $0^{m}\cdot 76$, we have when t the mean temperature of the external air is $+16°$, the index of refraction

$$= \sqrt{1+0\cdot 000554}, \text{ and}$$

at $+80°$, or the mean temperature of the internal air, the index will be

$$= \sqrt{1+0\cdot 0004529}$$

Whence, if ν is the ratio between these two elasticities,

$$\nu = 1\cdot 000051.$$

If Δ is the deviation produced by the prism, and $\delta\Delta$ the small angle through which the ray deviates still more in passing through the plate of mica, we have

$$\text{Sin } \Delta = \nu \,.\, \sin(\Delta - \delta\Delta), \text{ or}$$

$$\delta\Delta = \frac{\nu - 1}{\nu} \,.\, \text{tang } \Delta$$

which correction ought always to be added to the observed deviation. Calling, in short, $\pm d\Delta$ the variation produced in the deviation corrected by $\delta\Delta$, and putting $\varepsilon =$ the refracting angle of the prism $\pm\, d\varepsilon =$ the variation of this angle, and $n =$ the index of refraction, we shall have with a sufficient approximation, for crystals with one optic axis,

$$n = \frac{\sin \frac{1}{2}(\Delta \pm d\Delta + \varepsilon)}{\nu \,.\, \sin \frac{1}{2}\varepsilon},$$

and for crystals with two optic axes,

$$n = \frac{\sin \frac{1}{2}(\Delta \pm d\Delta + \varepsilon \pm d\varepsilon)}{\nu \,.\, \sin \frac{1}{2}(\varepsilon \pm d\varepsilon)}.$$

The results of the observations were as follows:

1. *Calcareous Spar.*—Refracting angle of prism $=59°\ 55'\ 9''$.

a. The Ordinary Spectrum.—For this spectrum I found the remarkable property, *that the crossed wires of the telescope being placed in the ray* F, *at a low temperature, they always remained fixed there at the highest temperature, notwithstanding a difference of* 64°.

The variation which sometimes presented itself during the repetition of the observation was too small to be measured. It is besides evident that the slightest change in the refracting power, would have produced a remarkable change in the deviation, which was $= 52°\ 53'\ 43''$. *This apparent invariability of the deviation proves a small decrease in the refracting power;* because if the latter had been perfectly constant, the observation would have shown an augmentation of

the deviation, for the density of the surrounding air had become less. It is easy to calculate how much this augmentation would have been. The index of the ray F being at the ordinary temperature = 1·66802, the deviation Δ calculated by the formula $\sin \frac{1}{2} (\Delta + \varepsilon) = 1{\cdot}000051 \,.\, + 1{\cdot}66802 \,.\, \sin \frac{1}{2} \varepsilon$, becomes = 52° 54′ 14″, or greater by 31″ than 52° 53′ 43″.

From these 31″ we must subtract $\frac{0{\cdot}000051}{1{\cdot}000051}$ tang 52° 53′ = 14″,

or the angle which the ray deviates at the plate of mica, so that there will remain only 17″. Though this quantity is the least which I could directly measure with the repeating circle, I have no reason to believe that if it did occur, it would have escaped me. Whence we may conclude, *that the refractive power of calcareous spar for the ordinary ray, either does not change at all with the temperature, or decreases with it by a quantity extremely small.*

b. The Extraordinary Spectrum.—The deviation of the ray F was found to be augmented by a difference of temperature of 64°, a quantity = 2′ 26″.

If we add to this the correction for the passage of the light

through the plate of mica = $\frac{0{\cdot}000051}{1{\cdot}000051}$ tang 36° 18′ 26″

= 8″·0, *the total augmentation produced by the temperature in the deviation of the extraordinary ray becomes*

= 2′ 34″

The calculation gives the index = 1·49118, whereas at the ordinary temperature it was = 1·49075. Hence it follows, *that a difference of temperature of 64° produces in the index of refraction of the extraordinary ray of calcareous spar*, an increase of +0·00043.

During my stay at Berlin in the month of May, of the present year (1832), I had fortunately an opportunity of confirming, at the house of M. Mitscherlich, and in his presence, this remarkable property of calcareous spar,—that the deviation of the ordinary ray does not change, at least not in an appreciable manner, with the temperature; while, on the contrary, that of the extraordinary ray increases considerably with the temperature. This property is in a certain manner connected with the discovery of M. Mitscherlich,—that *calcareous spar, when the temperature rises, dilates itself in the direction of the axis of crystallization, but undergoes in a direction perpendicular to the axis a contraction extremely small.* The crystal thus approaches to a cube with an increase of temperature, and the double refraction ought, consequently, to diminish, as

my observations prove. But, on the other hand, it appears singular, that while the dilatation of bodies commonly diminishes their refractive power, the extraordinary ray, though the crystal is dilated in the direction of its axis, becomes nevertheless less refracted, and that the refraction of the ordinary ray, notwithstanding the contraction of the crystal in a direction perpendicular to its axis, does not change, or diminishes if there is any variation. An analogous phænomenon, however, has already been observed by M. Arago in water *, in which refraction always goes on increasing from the temperature of the maximum density to the point of congelation.

2. *Rock Crystal.*—Refracting angle of the prism $=45° 20' 5''$. In both spectra a decrease of the deviation was observed, which was also sensibly the same; viz.

$$= 48''\cdot 0$$

whence, on account of the correction $= \frac{0\cdot 000051}{1\cdot 000051}$ tang $28° 15'$ $= 6''\cdot 0$, it follows that the total diminution of the deviations in both spectra is

$$= 42''\cdot 0.$$

The indices calculated for the ray F become in the extraordinary spectrum $=1\cdot 55868$, or $0\cdot 00028$ less than at the ordinary temperature; and in the ordinary spectrum, $=1\cdot 54944$, or $0\cdot 00026$ less.

3. *Arragonite.* — The experiments were made with the prisms A, No. 1; A, No. 2; B, No. 2; and C, No. 2.†

In all of them I found for the spectrum polarized perpendicular to the axis of crystallization a diminution, of the deviation produced by an increase of temperature. I also observed a change in the refracting angle of the prisms, with the exception of prism A, No. 1; with which, in this respect, on account of the magnitude of the refracting angle, no experiment with the heating apparatus could be made. The following were the observed results:

	A, No. 1.	A, No. 2.	B, No. 2.	C, No. 2.
Variation of the deviation . . .	$-5' 8''$	$-1' 53''$	$-4' 3''$	$-2' 58''$
Variation of refracting angle	not measured.	$+16''\cdot 0$	$-1' 53''$	$-48''\cdot 6$

If we correct these values for the deviation of the plate of mica, we obtain

* See Edinb. Encyclopædia, Art. Expansion, vol. ix. p. 257; and also the observations in the next page.—Edit.

† See present volume, p. 139.—Edit.

	A, No. 2.	B, No. 2.	C, No. 2.
Real variation of deviation	$-1' 47''$	$-3' 57''$	$-2' 52''$
Real variation of refracting angle	$+30''\cdot0$	$-1' 44''$	$-40''$

Whence we obtain the following indices of refraction:

A, No. 2.	B, No. 2.	C, No. 2.
1·53416	1·69421	1·68976

At the ordinary temperature of the air they were

1·53478	1·69510	1·69058

Thus the diminutions which an increase of temperature of 64° has produced in the indices of refraction in the spectra polarized perpendicular to the axes of crystallization, are

A.	B.	C.
−0·00062	−0·00089	−0·00082

The double refraction of arragonite thus appears to decrease a little with the temperature, because the refracting power in the direction of the axis A has diminished in a smaller ratio than that according to the axes B and C. In other respects arragonite comports itself quite differently from calcareous spar: the axis A of arragonite obviously corresponds with the axis of crystallization of the spar; but notwithstanding this, the refracting power in this direction diminishes in the former, and, on the contrary, increases in the latter; besides that in the direction perpendicular to the axis A, the refracting power diminishes considerably in arragonite, whilst, on the contrary, it undergoes almost no change in Iceland spar.

Observations on the preceding Paper.

The optical readers of this Journal will, we are sure, join with us in expressing our obligations to Professor Rudberg, for the accurate and valuable observations contained in the preceding communication, which he has been so kind as to transmit to us. The subject is entirely new, and we trust that he will extend his researches to other minerals, and also to artificial salts. The influence of heat in modifying the refractive power of uncrystallized solids, such as glass, gums, &c.; of fluids, such as water, oil, &c.; of fluids with circular polarization, such as oil of turpentine, &c.; and of minerals, &c. belonging to the tessular system, such as rock salt, alum, &c.—merit the attention of Professor Rudberg.

In reference to the important observation of M. Arago, that the refractive power of water gradually increases while it passes from that of its maximum density to that of congelation, we beg leave to quote the following observations*.

" When the writer of this article had the pleasure of seeing

* Art. Expansion, Edinb. Encyclopædia, vol. ix. p. 257.

M. Arago at Paris, in the course of last summer (1814), he mentioned to him a series of experiments on the refractive power of water at different temperatures, in order to determine if its maximum density was above 32°. He filled a prism with water at the temperature of 32°, and observed the angle of deviation produced by refraction, while its temperature rose from 32° to 212°. The angle of deviation was greatest at 32°, and it gradually diminished to 212°, exhibiting no marks whatever of a variation of refractive power at 40°, or at any point between 32° and 212°. Hence M. Arago concluded, that since the refractive power always increases with the density, the density of water must be a maximum at 32°. * * * It is assumed in this reasoning, *that the refractive power of bodies increases with their density*,—a doctrine which requires to be established by direct experiment, before it can be admitted as a valid argument in favour of any other position. Nay, it has actually been proved by Albert Euler, from numerous experiments, that the refractive power of glass is *increased by heat.* An augmentation of temperature of 60° of Reaumur diminished the focal length $\frac{1}{65}$th part, and an augmentation of 33° produced a diminution of $\frac{1}{97}$th. M. Euler concludes, without sufficient evidence, that the refractive power of fluids is increased with heat."

Looking at all these facts together, the action of heat is very anomalous:

Heat increases the refractive power of glass.

Heat diminishes the refractive power of water, oils, &c.

Heat increases the extraordinary refractive power of calcareous spar.

Heat diminishes the extraordinary refractive power of quartz.

Heat does not affect the ordinary refractive power of calcareous spar.

Heat diminishes the ordinary refractive power of quartz.

Hence there is reason to infer that heat produces some other change in the state of a body than a mere change in the relative distance of its particles.

The difference between the action of heat on *calcareous spar* and *quartz* is very extraordinary. The primitive form of each is a rhomb; and they differ only in the former having *negative*, and the latter *positive* double refraction. It will, therefore, be of importance to examine other negative and positive crystals; and if the difference of effect is not found to depend upon this circumstance, it may possibly arise from the peculiar structure of quartz in reference to circular polarization.

Among the other crystals which M. Rudberg will doubtless examine, we trust he will not omit *sulphate of lime* and *glauberite*, on the doubly refracting structure of which, heat produces such extraordinary effects.—D. B.

LXXI. *On the Action of Heat in changing the Number and Nature of the Optical or resultant Axes of Glauberite.* By Sir David Brewster, *K.H. LL.D. F.R.S. V.P.R.S. Ed.*

SEVERAL years ago Prof. Mitscherlich made the beautiful discovery, "that the ordinary *sulphate of lime* or *gypsum* which, at common temperatures, has two optic axes *in* the plane of its laminæ inclined at 60° to each other, undergoes a great change by elevation of temperature; the axes gradually approaching each other, collapsing into one, and (when yet further heated) actually opening out again in a plane *at right angles* to the laminæ."

Sir John Herschel, in whose words we have described this remarkable experiment, goes on to observe, "This singular result we cite from memory, having in vain searched for the original source of our information; but it might have been expected, from the low temperature at which the chemical constitution of this crystal is subverted by the disengagement of its water, that the changes in its optical relations by heat would be much more striking than in more indestructible bodies. We have not, at this moment, an opportunity of fully verifying the fact; but we observe that the tints developed by a plate of sulphate of lime, now before us, exposed as usual to polarized light, rise rapidly in the scale when the plate is moderately warmed by the heat of a candle held at some distance below it, and sink again when the heat is withdrawn, which, so far as it goes, is in conformity with the result above stated. Mica, on the contrary, similarly treated, undergoes no apparent change in the position of its axes or in the size of its rings, though heated nearly to ignition*."

In repeating this important experiment, I made use of one of the specimens described in the Phil. Trans. for 1818, in which I discovered one of the resultant axes of this mineral. It was about $1\frac{1}{8}$ inch thick in the plane of the laminæ, and the system of rings which surrounded this axis was exceedingly minute, with the usual black brush at each end of them. The other system of rings could not be seen in this specimen, owing to the manner in which it was cut. Having brought the crystal to a considerable heat, and exposed it to polarized light, it

* Treatise on Light, Encyclop. Metrop. p. 568.

was a singular sight to see the system of rings travelling along towards the line which bisects the optic axes, like a celestial body passing through the field of a telescope, and changing their form and size as they advanced. The specimen did not permit me to see the two systems unite, and still less to see them open out again in a plane at right angles to the laminæ; but from the degree of heat which I used, and which drove off the water of crystallization from part of the specimen, I presume that the complete phænomenon cannot be developed without destroying the constitution of the crystal; that is, that after the two systems of rings have opened out in a new plane, they will not return by cooling, through their state of union, into their primitive inclination of 60° in the plane of the laminæ.

A property of a similar kind, but perhaps a still more extraordinary one, I discovered some years ago, subsequent to Professor Mitscherlich's discovery; and I have slightly noticed it in a paper on Glauberite, published in the Edinburgh Transactions*. This interesting mineral has at ordinary temperatures the curious property of *two axes of double* refraction for *red* light, and only *one axis* for *violet* light. If we apply heat to it, the two optic axes for red light gradually close, and, at a temperature which the hand can endure, the two systems of rings for red light have united into one system, so that the crystal has now only one axis of double refraction for red light. By continuing to increase the heat the two axes separated, and the single system of rings opened out into two systems lying in a plane at right angles to that in which they were placed at first. The heat was now less than that of boiling water. By increasing it, the inclination of the optic axes gradually increased.

I now applied artificial cold to a crystal of glauberite at the ordinary temperature of the atmosphere. The inclination of the optic axes for red light increased, as might have been predicted; but, what was very unexpected, *a new axis was created for violet light*, the plane of the two violet axes being coincident with the plane of the two red optic axes at and below the ordinary temperature. An increase of cold increased the inclination of the optic axes for all the colours of the spectrum; the inclination of the axes being *least* for the *most refrangible*, and *greatest* for the *least refrangible* rays.

These results appear very complicated when we begin with the effects at an ordinary temperature, and view them in the manner in which they were observed; but if we commence the experiments at a low temperature, such as the freezing

* Edinb. Phil. Trans. vol. xi. Part ii. p. 273.

point, the order and connexion of the phænomena will be more easily understood.

At 32° glauberite has two axes of double refraction for rays of all colours, the inclination of the axes for the violet rays being least, and that for the red the greatest. As the temperature rises, the optic axes for all colours gradually approach, and the axes for violet first unite into one. At this time the crystal has two axes for all the other colours; but as the heat increases, all the other pairs of axes unite in succession, and form a single system of rings. But before this has taken place, the axes for violet rays have opened up again in a plane at right angles to that in which they originally lay, and they are followed by all the other pairs of axes; so that at a temperature much below that of boiling water, each pair of axes appears with different inclinations arranged in a new direction.

During all the changes which have been described above, the crystal has preserved its constitution, and by abstracting the heat, the phænomena are all repeated in an inverse order.

If the crystal should happen to be observed at that temperature, which very often occurs, when the greenish-yellow or most luminous rays have the optic axes corresponding to them united, or form a single system of rings, then the blue rays will have two systems of rings lying in one plane, and the red rays also two systems of rings in a plane at right angles to this. In two rectangular positions, namely, when the planes of the double axes coincide with, or are at right angles to, the plane of primitive polarization, the black cross will be very distinct, but in intermediate positions it will be much less so, and the uniaxal system of rings which predominates, from the greater intensity of their light, will have that indistinctness of character which, whenever it occurs, indicates a peculiar action of the doubly refracting force on the differently-coloured rays. When the black cross is perfect and equally distinct in all positions, while the colours of the rings deviate from those of Newton's scale, then the axes for all colours are obviously coincident, and the peculiarity in the colour of the rings is owing to an irrationality in the action of the doubly-refracting forces on the differently-coloured rays. This deviation from the tints of Newton's scale, I have found in many crystals which have only one axis of double refraction. It is extremely common in crystals with two axes.

I have elsewhere described the construction of a *chromatic thermometer*, in which the temperature is indicated by the polarized tints transiently developed by heat in a number of plates of glass;—but it is obvious that a plate of glauberite

may be made a thermometer which will indicate by its change of tint very slight changes of temperature. The temperature at which a ray of definite refrangibility has the optic axes corresponding to it united, so as to form a single system of rings, is a point as well fixed as that of boiling water, and every different inclination of the optic axes of definite rays indicates two different temperatures in the scale of heat, equidistant from those other points at which the same rays have their axes united.

The accurate measurement of these angles would no doubt be difficult, but an instrument might be made to show them by inspection. The temperatures, however, might be more simply indicated by the great variety of tints successively developed by heat; and as each tint has a numerical value in the scale of colours, its accuracy would not be much less than that of the other method.

Allerly, Nov. 3rd, 1832.

LXXII. *An Account of Experiments with an Invariable Pendulum, during a Russian Scientific Voyage. By Captain* Luetke*.

THE observations of the invariable pendulum occupied the first place amongst the scientific researches to which our attention was directed during the circumnavigation of the *Séniavine*. All these observations are already calculated: but, as I have not yet been able to give them the form in which they will ultimately appear before the public, and as a new appointment confines me at present to other duties, I trust that a summary account of these observations and their results will be acceptable to the Imperial Academy of Sciences, as well as to the scientific world in general.

The apparatus, which we made use of in these experiments, is the same as that which had been previously adopted by Capt. Basil Hall, at the several stations in South America. It is, in fact, the same in construction as that which was employed by Capt. Sabine in his voyage to Spitzbergen. Before quitting England, a series of experiments was made at the Observatory at Greenwich: and again on our return. The second series gave a result, which differed from the first, about $\frac{6}{10}$ths of a vibration, in excess; which I attribute to a slight wearing of the knife edge. This difference ought perhaps to be distributed over the whole interval, in arithmetical pro-

* Translated from the *Bulletin Scientifique*, page xi., attached to the Memoirs of the Imp. Acad. of Sciences at St. Petersburgh. Series vi. vol. i. (1830).

gression : but I shall content myself, for the present, by taking the mean of the two results.

The other stations were Valparaiso, Sitka, Petropaulouski; the islands of Ualan, Guam, Bonine and St. Helena; and lastly, the Observatory of St. Petersburgh. Here the experiments had for their object, as much the length of the pendulum, as the change of length from temperature. The two series (one of which was made at the mean temperature of 31°·5 Fahr., and the other at 82°·5) showed a difference of 0·458 vibration in a mean solar day, for each degree of the scale. This result is 0·033 greater than that found by Capt. Sabine, from a similar process; although the two pendulums were made of the same kind of metal (bell-metal*), and were nearly of the same dimensions. But, I do not find, in our experiments, any thing that should cause this difference, unless it be the smaller density of the metal of which our pendulum is composed.

In voyages of this kind, the stay at each port is generally very short; and the time which we can give to each species of observation naturally very limited. It is evident therefore that we depend much on accidental circumstances, which may influence the success of our labours very considerably. Hence it happens that, although we have always paid the same attention to every thing that could contribute to the accuracy of the experiments, yet they are not all of the same value. Those which deserve the greatest confidence are those that were made at Greenwich, St. Petersburgh, Petropaulouski, Valparaiso, and the Bonine isles. At these five stations, I think I can answer for $\frac{1}{10}$th of a vibration. Then come Sitka, and the island of Ualan; where the mean result may be uncertain to $\frac{1}{4}$th of a vibration. The experiments, which are the least to be depended upon, are those made at the islands of Guam and St. Helena; where I do not pretend to a precision greater than $\frac{1}{2}$ a vibration.

The latitudes were determined by circum-meridian altitudes of the sun and stars, observed with a sextant and a circle, each by Troughton, and with a reflecting repeating circle by Dollond. We had not an astronomical repeating circle: but, we endeavoured, by multiplying the observations, and varying the circumstances, to make the above-mentioned instruments serve the same purpose.

I now come to the results; which are contained in the following Table: where the 3rd column contains the number of

* We suspect that Capt. Luetke is wrong in designating the pendulum as made of *bell-metal*; as we believe they are all made of *brass*.—Edit.

vibrations made by the pendulum, at each station, in a mean solar day, reduced to the standard temperature of 62° Fahr., to a vacuum, and to the level of the sea. The 4th column contains the corresponding length of the seconds pendulum (in English inches), founded on that determined by Captain Kater in London. For this purpose we ought to reduce the experiments at Greenwich to the station at Portland Place, by the difference in the number of vibrations, found to exist between these two places, by Capt. Sabine.

Stations.	Latitudes.	Number of Vibrations.	Length of the Seconds Pendulum.
Ualan............	5°21′ 16″ N.	86112·83	39·02756
Guam............	13 26 21 N.	117·98	·03242
St. Helena	15 54 59 S.	125·63	·03933
Bonine..........	27 04 12 N.	159·24	·06980
Valparaiso	33 02 30 S.	165·33	·07533
London	51 31 08 N.	235·80	·13929
Petropaulouski	53 00 53 N.	245·83	·14838
Sitka.............	57 02 58 N.	257·44	·15810
St. Petersburgh	59 56 31 N.	269·08	·16950

In order to find the mean result of the compression of the earth, corresponding to the whole of these experiments, we must adopt the method of least squares. If we denote by x and y the length of the pendulum at the equator, and the difference of that at the pole; by E′, E″, E‴ &c. the errors of each partial result; and recollecting that in the hypothesis for the elliptical figure of the earth the length of the pendulum for each latitude is $L = x + y \times \sin^2 L$; we shall have the following equations of condition: viz.

Ualan	$39{\cdot}02765 - x - 0{\cdot}0087080 \times y = E'$.
Guam	$39{\cdot}03242 - x - 0{\cdot}0540157 \times y = E''$.
St. Helena . .	$39{\cdot}03933 - x - 0{\cdot}0752045 \times y = E'''$.
Bonine	$39{\cdot}06980 - x - 0{\cdot}2070967 \times y = E^{iv}$.
Valparaiso . .	$39{\cdot}07533 - x - 0{\cdot}2972962 \times y = E^{v}$.
London . . .	$39{\cdot}13929 - x - 0{\cdot}6127966 \times y = E^{vi}$.
Petropaulouski	$39{\cdot}14838 - x - 0{\cdot}6380657 \times y = E^{vii}$.
Sitka.	$39{\cdot}15810 - x - 0{\cdot}7041567 \times y = E^{viii}$.
St. Petersburgh	$39{\cdot}16950 - x - 0{\cdot}7491220 \times y = E^{viiii}$.

The equation of the minimum, with respect to x, will be the sum of these same equations divided by their number: that is,

$$39{\cdot}095532 - x - 0{\cdot}3718291 \times y = 0.$$

In order to take the equation of the minimum with respect

to y, we multiply each equation by the co-efficient of y in this equation; which gives the following results: viz.

$$\begin{array}{rlll}
- & 0{\cdot}3398532 & + x \times 0{\cdot}0087080 & + y \times 0{\cdot}0000758 \\
- & 2{\cdot}1083632 & + x \times 0{\cdot}0540157 & + y \times 0{\cdot}0029177 \\
- & 2{\cdot}9359321 & + x \times 0{\cdot}0752045 & + y \times 0{\cdot}0056557 \\
- & 8{\cdot}0912268 & + x \times 0{\cdot}2070967 & + y \times 0{\cdot}0428890 \\
- & 11{\cdot}6169468 & + x \times 0{\cdot}2972962 & + y \times 0{\cdot}0883850 \\
- & 23{\cdot}9844280 & + x \times 0{\cdot}6127966 & + y \times 0{\cdot}3755198 \\
- & 24{\cdot}9792400 & + x \times 0{\cdot}6380657 & + y \times 0{\cdot}4071279 \\
- & 27{\cdot}5740089 & + x \times 0{\cdot}7041567 & + y \times 0{\cdot}4958368 \\
- & 29{\cdot}3427299 & + x \times 0{\cdot}7491220 & + y \times 0{\cdot}5611894 \\
\hline
- & 14{\cdot}5525254 & + x \times 0{\cdot}3718291 & + y \times 0{\cdot}2199552 = 0.
\end{array}$$

The sum, divided by 9, is the equation of the minimum with respect to y. Eliminating between these two equations, we have $x = 39{\cdot}02422$, $y = 0{\cdot}091787$, and $\frac{y}{x} = 0{\cdot}00865052$ $= \frac{1}{267{\cdot}7}$ = the compression.

If we substitute these values of x and y in each of the equations, we shall have the respective values of E′, E″, E‴ &c., or the difference of each partial result from that which corresponds to the mean of all the experiments, as they stand in the following table.

Stations.	Length of the Pendulum by Experiment.	Length of the Pendulum $x+y\,.\sin^2 L$.	Difference in	
			Length.	Vibrations.
Ualan............	39·02765	39·02589	+0·00176	+1·94
Guam............	·03242	·03458	−0·00216	−2·38
St. Helena......	·03933	·03864	+0·00069	+0·76
Bonine..........	·06980	·06394	+0·00586	+6·46
London	·13929	·14175	−0·00246	−2·71
Valparaiso	·07533	·08124	−0·00591	−6·52
Petropaulouski	·14838	·14659	+0·00179	+1·97
Sitka	·15810	.15927	−0·00117	−1·29
St. Petersburgh	·16950	·16789	+0·00161	+1·77

The greatest differences fall on Valparaiso and the Bonine islands. Fortunately these two stations are amongst those where the experiments were the most satisfactory: so that there can be no question about the errors of observation, in seeking for the cause of the difference; which must therefore be attributed to the anomalies of local attraction.

If, for the purpose of confining our remarks to the Northern hemisphere, we exclude Valparaiso, as the only station situated

very considerably to the South, we shall find $x = 39{\cdot}02522$, $y = 0{\cdot}191100$, and $\frac{y}{x} = \frac{1}{266{\cdot}4}$.

Lastly, if we exclude also the Bonine islands, where it appears there is a great degree of attraction, we shall have $x = 39{\cdot}023923$, $y = 0{\cdot}192535$, and $\frac{y}{x} = \frac{1}{269}$. And this is the result to which we have at length come, as best representing the whole of our experiments in the Northern hemisphere. In this case the differences of the partial results from the calculation will be as follow: viz.

Stations.	Experiments.	Calculations.	Difference.
Ualan............	39·02765	39·02560	+ ·00205
Guam............	·03242	·03442	— ·00200
St. Helena......	·03933	·03840	+ ·00093
London	·13929	·14191	— ·00262
Petropaulouski	·14838	·14677	+ ·00161
Sitka.............	·15810	·15950	— ·00140
St. Petersburgh	·16950	·16816	+ ·00134

The compressions which we have deduced are almost identical with the mean results of the experiments made by Captains Freycinet and Duperrey: but they are greater than those of Capt. Sabine. In order to determine whether these discordancies are to be attributed to local causes, to an irregularity in the form of the spheroid, or to errors of observation, it would be necessary to make other combinations by uniting the experiments not only of the navigators above mentioned, but also those of Captains Parry, Kotzebue and Hall: an undertaking which would exceed the bounds which I have prescribed to myself here, and which is reserved for the detailed account of our labours.

LXXIII. *On the Power possessed by Spiders to escape from an isolated Situation.* *By* George Fairholme, *Esq.**

HAVING observed, in a late Number of the Philosophical Magazine (August 1832), an article by Mr. Blackwall, in which a doubt appears to be expressed of the power of spiders to escape from an isolated situation by means of a projected thread, I beg leave to mention a few observations on that subject which were made by me some years ago, while

* Communicated by the Author.

residing in Switzerland, and which will place the matter beyond a doubt, in as far, at least, as it relates to one of the species, though not with respect to the *Aranea domestica*, or common house spider.

While residing on the shores of the Lake of Thoun, in the summer of 1828, I was frequently in the habit of spending some hours on the water, in a small boat, near a low part of the shore, where there was abundance of reeds growing in the water, and where those reeds gradually became more widely scattered, as the depth of the water increased, until at length they entirely disappeared.

I had frequently had occasion to remark, amongst the thicker crop of reeds, the singular manner in which the tops and stems of the plants were bound together by cobwebs of such strength and elasticity as to resist the action of the most powerful winds. But having observed that even the most distant and completely isolated plants were equally furnished with spiders and cobwebs, it became an interesting inquiry how the communication with these more distant objects was brought about, and what means of escape the little colonists had within their power; as I had never observed an instance of their passing along the surface of the water.

On taking, therefore, one of these spiders in my hand, I was not long in discovering their mode of operation. For when placed on the point of my finger, in an elevated position, I observed that a fine thread was proceeding in a rapid course, from the *loom*, and was carried by the wind to leeward, where it became attached to the first object with which it came in contact; and a communication was thus effected, by means of which the little captive was not long in making his escape.

Having thus discovered their general mode of operation, I had subsequently many opportunities of amusing myself and my friends, by more particular remarks and experiments on the powers of these curious insects. I have more than once taken spiders out into the lake, to endeavour to ascertain to what length this projected thread might be extended. In these experiments, tried in situations where a dark shade, as a back ground, enabled me to follow the course of the thread with my eye to some distance, it was always carried in about half a minute beyond my powers of vision, or to a distance of about twenty-five or thirty yards; and as no object intervened to which it could become attached, I have reason to think it might extend considerably further. On one occasion, I was enabled distinctly to trace the whole process, and the eventual escape of the spider to an object fully twenty yards distant.

I placed him on my finger, and with a microscope I observed the valves in the abdomen to open, by several distinct apertures, from each of which a fine thread of gummy liquid issued, all of which threads became united into one strong cord, which continued flowing until (carried by a gentle breeze to leeward) it became attached to the branch of a tree, about twenty yards distant.

It was highly interesting to observe the proceedings of the insect during the operation. He had previously, by simply bringing the lower extremity of his body in contact with my finger, attached the gummy thread firmly to it; and while it was flowing, which was distinctly visible by occasional enlargements in the thread (probably occasioned by dusty particles adhering to it as it flowed) he remained nearly still, except when making an occasional trial with one claw, to discover if it was yet fixed to any object. These trials strongly reminded me of those of a rope-dancer, while the assistants are screwing up his rope to the necessary degree of tension.

At length he seemed to have found the desired resistance, though I was not then aware of the object to which the line had become fixed. But a most singular operation now commenced, and was performed with extraordinary celerity. For by a rapid movement of his hooked claws, he "*hauled in the slack of the rope*," tightening it to the necessary degree; and when he had thus collected a confused mass of tangled thread, *he swallowed it*, and again fixing the tightened end of the cord to my finger, he lost no time in proceeding along the line towards the desired point. I now brought the cobweb in contact with a fixed object, near which I stood, and following the traveller (who was rocked by the breeze in a manner he would not have been if left to his own ingenuity, for in fixing the cord I had not attended with sufficient care to the degree of tightness to which he had himself arranged it), I saw him reach in safety the branch of a tree, fully twenty yards distant, to which I now found the projected thread had become fixed.

From the above observations, it is therefore clear that in some instances spiders are endowed with this remarkable power of escape; but whether this instinct is confined to those species which, from their abode near water, would appear most to require it, my observations do not enable me to decide. But it seems to me nearly certain that the gossamer which is seen floating in long threads in the summer air, is of the same nature as these projected cobwebs; and also that those innumerable and minute threads, which are often seen to cover the ploughed fields, and the hedges, in a horizontal

position, and which become so visible in the slight frost of an autumnal morning, partake of the same character.

Mr. Temple, in his amusing and interesting account of his travels in Peru, and while describing the first indications of the approach to the end of his voyage across the Atlantic, has the following passage, from which it would appear that instances similar to that which I have just described, may probably be found in various other parts of the world, and that the power of projecting a web of great length is possessed by more than one of the species.

" We weighed anchor, and made all sail up the stupendous, but wholly uninteresting river Plate, which is 120 miles wide at its mouth, and not less than from 20 to 30, for upwards of 150 miles inland. In the course of the day, the rigging of the ship, from top to bottom, was literally covered with *long fine cobwebs*, that had been blown off the shore, having attached to them their insect manufacturers, who dispersed themselves in thousands over our deck."—Travels in Peru, vol. i. p. 49.

I have frequently endeavoured to ascertain to what length one of these spiders had the power of spinning a thread. By letting the insect drop from an object of known dimensions held in the hand, and by winding out the thread, while turning the object, I thought it possible to form some idea of its length. But from the unnatural position of the spider, and the occasional force necessary to make him work, I have never been able to come to any definite conclusion on this point. But it appears to me probable, that from 30 to 40 yards is as much as can be produced at one time, without a degree of compulsion, or exhaustion, which makes him roll himself up, and become motionless for some time.

It seems certain that the substance from which the cobweb is composed is a gummy liquid while in the body of the insect, but becomes dry and elastic in the open air, in the same manner as threads of any other gummy substance when do drawn out in a half moist state.

Ramsgate, Oct. 5, 1832.

LXXIV. *Note on the Mean Temperature and Barometric Height of Sitka, on the North-west Coast of America. By Professor* M. A. Kupffer, *of the Imperial Academy of Sciences of St. Petersburgh**.

THE following meteorological observations have been communicated to me by M. Lutke. They will afford at least on approximate idea of the climate of Sitka.

* Communicated by the Author.

Table *containing the Maxima and Minima of the Barometric Height and Temperature for each Month of the Year* 1828 (*Old Style*).

Months.	Barometer in English Inches.		Reaumur's Thermometer.	
	Max.	Min.	Max.	Min.
January	29·80	29·20	+ 5	− 7
February	29·83	29·23	6	1
March	30·44	29·06	8	− 2
April..........	30·27	29·38	13½	+ 4
May	30·12	29·57	12	5½
June	30·20	29·45	16	7
July	30·20	29·75	18	9
August	30·17	29·34	15	5
September ...	30·20	29·20	12	+ 2
October	30·01	28·78	10	− 1
November ...	30·10	28·66	6	2
December ...	30·57	28·72	+ 6½	−10
Mean	30·16	29·20	+10·7	+ 0·8

If we take for *Jloulouk* the mean of the maxima and minima of the barometric heights of all the months of the year 1828 (see the second table of the following article), we shall find

	Engl. Inches.
Mean of maxima for Jloulouk	29·92
Mean of minima	28·80
Mean	29·36

To which, adding 0·32; that is to say, the error of the barometer, which gave the preceding means,— we have . . . . Inches. 29·68.

If we make the same calculation for *Sitka*, we shall obtain . . . . 29·68.

That is to say, exactly the same value. This agreement gives a greater weight to the observations than might otherwise be attributed to them.

It is easily seen that for Jloulouk the mean of the maxima and minima of all the months is greatly different from the true mean barometric height.

With regard to the thermometrical observations, we know also that the mean temperature of the year does not differ much from the mean of the maxima and minima of all the months.

The mean of these maxima and minima for Sitka, for the year 1828, as we see by the preceding table, is +5°·8 Reaum. or, ... 45°·05 Fahr.

It is not, however, without diffidence that I communicate this result.

LXXV. *Note on the Mean Temperature and Mean Barometric Height of Jloulouk, in the Island of Ounalachka. By Professor* M. A. Kupffer, *of the Imperial Academy of Sciences of St. Petersburgh**.

M. LUTKE has communicated to me some meteorological observations which have been sent to him from Ounalachka. Though these observations do not yet contain a very great space of time, I have no doubt that the interest which they possess will be received as an excuse for the hurry in which I have published them.

The following Tables contain the mean barometric heights, expressed in English inches, and the indications of a thermometer of Reaumur placed in the shade, and towards the north, in a hollow cylinder of white iron, open at both ends. The observations were made three times a day, about 8^h^ A.M., 1^h^ P.M., and 9^h^ P.M. The duties of the service did not always permit the observer to keep exactly to these hours; the observations were sometimes made half an hour sooner or later: but such is the slowness with which the temperature varies in this climate, that this irregularity does not sensibly affect the accuracy of the mean results.

Table I.—*Containing the Mean Temperature of each Month of* 1828, *and part of the Years* 1827 *and* 1829 (*Old Style*).

Mean Temperature. Reaumur.			Mean Temperature. Reaumur.			Mean Temperature. Reaumur.		
		°			°			°
1827.	Oct.	+1·7	1828.	May	+ 4·1	1828.	Dec.	−3·1
	Nov.	2·0		June	6·6	1829.	Jan.	1·5
	Dec.	1·5		July	8·4		Feb.	−0·4
1828.	Jan.	+3·7		Aug.	11·0		Mar.	+0·1
	Feb.	−0·1		Sept.	6·2		April	0·8
	Mar.	−0·1		Oct.	+ 2·9		May	4·1
	April	+2·1		Nov.	− 0·1		June	+6·6

Mean of the first twelve months... 4°·0
Mean of the year 1828 .... 3 ·5
Mean of the last twelve months ... 3 ·0
General mean .... 3°·5 or 39°·875 Fahr.

According to the observations made at Leith Firth in Scotland, and communicated by Sir D. Brewster, the mean observation made at 8^h^ A.M., 1^h^ P.M., and 9^h^ P.M., will exceed the true temperature of the place by 0°·2 of Reaumur.

Hence the corrected mean temperature of Jloulouk will be 3°·7 Reaumur, or 40°·325 Fahrenheit.

* Communicated by the Author.

Table II.—*Containing the Mean Height of the Barometer, and the Extent of its Variations, for each Month of the Year* 1828 *and part of* 1827 *and* 1829 (*Old Style*).

Months.		Mean Barometric Height in English Inches.	Maximum.	Minimum.	Differences of Max. and Min.
1827.	Oct.	29·33	29·85	29·01	0·84
	Nov.	29·44	30·08	28·60	1·48
	Dec.	29·65	30·26	28·87	1·39
1828.	Jan.	29·47	29·94	28·77	1·17
	Feb.	29·17	29·84	28·35	1·49
	March	29·42	30·08	28·72	1·36
	April	29·32	29·74	28·98	0·76
	May	29·50	30·06	28·94	1·12
	June	29·44	29·78	28·96	0·82
	July	29·56	29·82	29·18	0·64
	August	29·65	30·00	29·20	0·80
	Sept.	29·41	29·77	28·74	1·03
	Oct.	29·16	29·82	28·45	1·37
	Nov.	29·20	29·85	28·66	1·19
	Dec.	29·83	30·38	28·71	1·67
1829.	Jan.	29·29	29·73	28·36	1·37
	Feb.	29·20	29·69	28·55	1·14
	March	29·08	29·98	28·51	1·47
	April	29·55	30·24	28·44	1·80
	May	29·43	30·11	28·80	1·31
	June	29·55	29·89	29·05	0·84
Mean ...		29·41	29·95	28·75	1·19

The barometer used in the preceding observations was compared with that of M. Lutke. He found that the former constantly indicated a height 0·32 inch less than the latter. The barometer of M. Lutke having been compared with that of the observatory of Copenhagen, its indications may be regarded as exact.

Adding 0·32 to the above mean, we find for the mean barometric height of Jloulouk* 29·73. The temperature of the mercury was unfortunately not observed. We may, however, without falling into a great error, regard the above barometric height as reduced to + 14° Reaum. This result confirms a remark made by M. Erman, jun., on the barometric height of the sea of Okhosk. See Poggendorf's *Annalen* 1829, No. 10.

* See the preceding article Note on the Temperature, &c. of Sitka.—Edit.

Table III.—*Showing the State of the Winds observed thrice a day.*

During the past year there were,

92 North winds	85 West	170 South	23 East
49 NNW	45 WSW	34 SSE	6 ENE
59 NW	106 SW	49 SE	42 NE
32 WNW	41 SSW	15 ESE	21 NNE.

Hence we see that the prevailing winds are those of the *south* and the *south-west*.

LXXVI. *Observations on the Isothermal Lines on the North-west Coast of America, as deduced from the Results in the two preceding Articles. By* Sir David Brewster, *LL.D. F.R.S. &c.*

IN determining the inflexions of the isothermal lines round the pole of maximum cold in the Arctic regions to the North of America, I employed the valuable observations of Mr. Scoresby; a long and valuable series of observations made on the west coast of Greenland, and communicated to me by Sir Charles Giesecke; together with observations made in Iceland, and in different parts of Canada. I sought in vain, however, for measures of mean temperatures in those parts of the Arctic regions which are placed in a meridian nearly opposite to our own; and it is therefore a source of great satisfaction to me to have received from M. Kupffer the valuable observations contained in the two preceding papers. These observations indeed have been yet made for too short a period to give us a very accurate measure of mean temperature; but the approximate results which they afford will be of some use, till we obtain a larger series.

In order to compare the observed mean temperatures of *Jloulouk* and *Sitka* with those calculated from the formula

$$\text{Mean temperature} = (86°{\cdot}3 \sin D) - 3\tfrac{1}{2},$$

D being the distance of the place of observation from the North American pole of maximum cold, which is situated in north latitude 80°, and west longitude 100°.

Professor Kupffer has not given us the longitude and latitude of Jloulouk and Sitka. The position of Ounalachka, however, according to the observations of English navigators, is between 168° 40′ and 168° of west longitude, and between 53° 45′ and 54° of north latitude. We shall take, there-

fore, the position of Jloulouk at 168° 20′ west longitude, and 53° 53′ of north latitude; and from these data we shall find . . . . D = 33° 23′

And the calculated mean temperature of Jloulouk	43°·980
Observed mean temperature	40 ·325
Difference	+3 ·655

This difference between the formula and observation is much greater than usual; but we shall presently see that it must arise either from the observations not affording a correct mean, or from the temperature of the place being affected by local causes.

I presume that Sitka is the same place as the Isle of Sitka, iu the Great Northern Ocean, where Dr. Erman made his magnetical observations. The following extract from Dr. Erman's table, given in his letter to the academician M. Wisniewsky, and published in the *Bulletin Scientifique*, will enable us to approximate to the position of Sitka.

	North Lat.	West Long.
Nov. 4. In the Great Northern Ocean	56° 54′·20	223° 53′·20
— 12. At the Isle of Sitka	57 3 ·12	
— 20. In the Great Northern Ocean	54 26·50	221 22 ·80

As Dr. Erman has omitted to give the longitude of Sitka, we may infer from the preceding table that it cannot be far from 222°.

Hence we obtain D = 25° 38′, and

The calculated mean temperature of Sitka . .	33°·84 Fahr.
Observed mean temperature	45 ·05
Difference	—11 ·21

This difference is so extraordinary, that we must either have mistaken the locality of Sitka, or there must be some singular source of heat in the island, or some inexplicable error in the observations. The reader will observe, that the difference is now —, whereas it was + in the case of Jloulouk.

Without taking the formula as our guide, we have only to consider that Jloulouk, in latitude 53° 53′, has only a temperature of 40°; while Sitka, in latitude 57° 3′, has a temperature of 45° ! in order to be convinced that the formula will not be found to be in fault. We must wait, therefore, for other observations from these regions before we can explain the cause of this singular discrepancy.

LXXVII. *Further Remarks on Experiments relative to the Interference of Light. By the Rev.* Baden Powell, *M.A., F.R.S., Savilian Professor of Geometry, Oxford*.*

IN a paper which appeared in the Philosophical Magazine and Annals, N.S. for January 1832, I described a few experiments, having in view the fuller illustration of the principle of interferences, and of the undulatory theory of light. Those remarks claim no higher character than that of attempts to simplify some points in the inquiry, and facilitate the diffusion of elementary knowledge on this most beautiful and interesting branch of science; and it is with no other object that the present paper has been drawn up.

There are, in fact, few parts of science (especially considering the experimental and popular form of which it is susceptible,) which have excited less general attention than that to which the name of *physical optics* has been applied. And I would observe, in passing, that as the distinction implied in this designation between the properties of interference, polarization, &c., and those of ordinary reflexion and refraction is *wholly arbitrary*,—these last being just as much *physical* properties as the former,—it would surely be better to restrict the term "*physical* optics" to a sense analogous to "*physical astronomy*," viz. to the theory of those motions and forces which shall account for the observed effects on dynamical principles. This is quite independent of the facts. Nor is the distinction merely verbal, for I believe that an imagined inseparable connexion between the facts of the coloured rings, polarization, &c. and the physical theory, has tended in no small degree to create a reluctance in many persons to enter upon a subject which was supposed to be identified with a doubtful, abstruse, and difficult hypothesis.

In this point of view, therefore, the distinction may be by no means unimportant. Long since, indeed, those who were most profoundly versed in the subject were not backward in their attempts to dispel such misconceptions. The present Lord Chancellor Brougham, in his highly acute and original papers "On the Reflexion, Inflexion, and Colours of Light," in the Philosophical Transactions for 1796–97, insisted strenuously on the distinction between the *facts* of Newton's experiments (which he pursued and extended) and the *theories* whether of fits or waves; and though he rejected both for a simpler theory of his own, this was at a period when the real nature of either was as yet undemonstrated. And when the singular fact of interference was unequivocally established by Dr.

* Communicated by the Author.

Young, it is surprising that his lectures and writings did not produce a general impression in favour of a branch of science, which his researches had successfully reduced to a form not less philosophically comprehensive, than experimentally simple and striking. More lately Fraunhofer, in his elaborate researches, has carefully pointed out the distinction that these "*intervals*" have a *real existence*, whatever theoretical view we may take of their nature. The publication of Sir J. Herschel's Treatise on Light forms an epoch in the history of the science, and has given a material impulse to the study of it: and there are only wanting further endeavours to simplify and facilitate the investigations, and to bring them more within the reach of the generality of students, in order to diffuse a knowledge of the subject as widely as its beauty and importance demand.

The only point which continued to be a matter of question, viz. the application of interferences to the colours of thin plates, having now been decisively settled by Professor Airy's *experimentum crucis* (Phil. Mag. and Annals, August 1831), the language of comparison between the theories which was legitimately used before, has ceased to be admissible. But the sagacity with which Newton detected the existence of these intervals, as well as the accuracy with which he measured them, only continues to be enhanced in our estimation by the more recent explanation of their nature. He observed that at certain thicknesses no sensation of light *reached the eye:* it was therefore a natural and unavoidable conclusion, that none was *reflected*,—when as yet neither the paradoxical fact that two reflexions might destroy each other, nor the equally essential point that in this case there were two reflexions concerned, had been established.

Next to the fact of the *existence* of the intervals, that of their *diminution* in more refractive media is of fundamental importance to the theory. The fact is indeed involved in a variety of phænomena: but as I have not happened to meet with any detailed account of the method of showing it experimentally, except mixed up with other considerations, I will here offer a few remarks upon the subject.

In the first place it is evident, that if we employ the inclined reflectors for the interference-stripes*, and they be immersed in a transparent fluid, the surface of the liquid medium being so arranged that the rays are incident perpendicularly, and if the eye-lens be placed in such a manner as to receive the rays

* The experiments, as well as the formula here referred to, are those described in my former paper.—Phil. Mag. and Annals, N.S. vol. xi. p. 4, &c.

immediately from the liquid, the stripes will be seen as formed at its focus within the liquid. This will be immediately understood on recollecting that the stripes seen by a lens, under any circumstances, are those formed *at its focus*, and are to be regarded simply as an optical image situated there.

Hence also, if the lens have not its surface in contact with the liquid, but be placed at any distance from the surface less than its focal length, it will still exhibit the stripes as formed within the liquid.

In this way the stripes will be seen narrower, or having less values of c than in air: and from the equation,

$$c = \frac{\lambda}{2} \cot \theta, \quad \text{or} \quad \lambda = 2c \tan \theta,$$

it follows, that since θ remains the same, λ must be diminished within the medium. If the obtuse prism be employed, or the interference be produced by refraction, it will be manifest that, owing to the less difference in refractive power between the glass and the liquid, than between the glass and air, the rays will cross at a smaller angle; and if λ remain the same, c ought therefore to be greater; but in point of fact the stripes are observed unaltered, or c remains the same: hence it follows that λ must really be diminished. The diminution of θ is evidently due to the difference of refractive power between the liquid and air; and if m be the relative index (calling the angle in the liquid θ), we have $\sin \theta = m \sin \theta$; and since in such very small angles we may substitute the sine for the tangent, the formula gives, in air, $\lambda = 2c \sin \theta$

$$\text{in the liquid, } \lambda_{,} = 2c \frac{1}{m} \sin \theta,$$

Or $\lambda_{,}$ is a value diminished exactly in the ratio of the refractive powers. Either of these experiments is, however, very troublesome to manage; and it is very difficult to compare satisfactorily the breadth of the stripes in the two cases. The following is much easier, and has the additional advantage of being applicable to solid transparent bodies as well as to liquids.

If a mass of a solid transparent body with plane parallel surfaces, or of a fluid bounded by parallel plates of glass, be simply placed between the prism or reflector and the eye-lens, the latter having its focus within the medium as before, then the rays on entering the medium, as they emerge from the prism inclined to each other at an angle 2θ, that angle will now be diminished to $2\theta_{,}$, and hence, as before, the stripes formed within the medium ought to become wider; but in point of fact they are seen *unaltered:* they may in this case be

easily compared with the stripes formed in air by arranging the lens so that one half of it is exposed to the direct rays, the other to those passing through the medium. The two portions of the stripes thus seen are, in fact, continued exactly in the same straight lines; and the eye would be well able to judge of the smallest deviation in such extremely delicate and well-defined lines, did any exist. The stripes, however, are precisely of the same breadth, or the values of c the same, whether with or without the interposition of the medium: hence, as before, λ must be diminished exactly in proportion as m is increased. But these experiments refer only to the fact, that along the length of what we term a ray of light, there occur at intervals equal to the value of $\frac{\lambda}{2}$, points at which the ray is in some way affected with a different character, such that the concurrence of two rays at a point of like affection shall produce light, but at one of opposite affection, darkness; this character evidently changing gradually from one condition to the other. All this, however, may consist with the notion of these different affections occurring simply at successive fixed points equidistant along the ray, which are merely more crowded together in a denser medium. It is then a separate conclusion, established by distinct evidence, that these intervals are propagated along the line which represents the ray with a definite velocity, diminished in proportion to the refractive power of the medium. The simple fact of the diminution of the intervals is, however, usually presented mixed up with the consideration of this progressive propagation, or rather of its retardation in denser media, in explaining the shifting of the stripes, as in the experiments referred to in my former paper, and to which the method I have suggested by means of a thin prism, offers a convenient auxiliary process; showing at once the existence of the effect and its amount. Connected with this subject, some curious researches were given by Mr. Potter, in a paper read before the British Association for the Advancement of Science, at the Oxford meeting in June last, to the publication of which I look with great interest, especially as they were founded on my experiment just alluded to, and as their tendency appeared to be somewhat at variance with the received views on this point*.

There are many students to whom it is an important object to know what is the least possible amount of apparatus indispensable for carrying on their experiments. It may be desirable to mention, therefore, that in order to perform the

* Mr. Potter's paper here alluded to will appear in our next Number.—Ed.

interference-experiments, although it is most convenient to have an apparatus like that of a solar microscope for sending the sun's rays horizontally into a dark room, and concentrating them by a small lens, whilst the mirror is capable of following the motion of the sun,—yet all these conditions are not absolutely essential. A completely darkened room is far from indispensable, and the apparatus may be perhaps more conveniently fixed in a screen, which may at any moment be placed in the sun's rays in any situation, and the effect observed at some feet distance. But the place of such an apparatus may be quite sufficiently supplied from materials everywhere at hand, by merely adjusting a common plane mirror in a proper position, and receiving the rays either through a small lens, or even a minute aperture in a screen; the sole essential being an origin of light, which is as nearly as possible an absolute point. Or, again, we may use still simpler means; for if we have only a small convex mirror, such as the bulb of a thermometer, a globule of mercury, a polished metallic button, &c. simply placed in the sun's rays, this gives an image of the sun diminished nearly to a point; and if the light from other sources be screened, the diverging beam thus formed will suffice for showing the stripes either with the obtuse prism, or by two reflecting surfaces, not indeed with the same brilliancy and distinctness, but sufficiently to verify the facts.

This method is also successfully applicable to the fringes of edges and apertures, and the internal stripes of shadows formerly called Diffraction. And mentioning these phænomena, I may observe, by the way, that the simple property on which the explanation of these fringes is founded, viz. the tendency of light to diverge from a new origin whenever an obstacle is presented,—which is a real exception, as far as it goes, to the primary law of the rectilinear propagation of light,—does not appear to me (except, perhaps, in one passage in Sir J. Herschel's treatise,) to have been placed in that prominent point of view in which it should be stated in the elementary exposition of the nature and propagation of light. There are also some other remarkable facts apparently dependent on it, which I am engaged in investigating.

One of the most singular experiments connected with the coloured fringes is that in which the centre of the shadow of a small circular disk appears a bright point. This experiment is difficult to perform satisfactorily; since even when such a disk is cut with the utmost care, each of the minute inequalities in its edge is magnified, and accompanied with fringes, which mix and cross in such a manner as totally to confuse the whole appearance. I have succeeded by taking up a

small quantity of thick ink on the point of a pin, and dropping it on a clear plate of glass, by which means a sufficiently even circular edge is produced, the disk being about $\frac{1}{10}$th inch diameter.

Oxford, Nov. 4, 1832.

LXXVIII. *Account of a curious Chinese Mirror, which reflects from its polished Face the Figures embossed upon its Back.* By Sir D. Brewster, *K.H. LL.D. &c.*

WE have just received, through the kindness of George Swinton, Esq. of Calcutta, whose zeal for the promotion of science is never relaxed, an account of a curious metallic mirror, which had been recently brought from China to Calcutta, and which was then amusing the dilettanti and perplexing the philosophers of our Eastern metropolis.

This mirror has a circular form, and is about five inches in diameter. It has a knob in the centre of the back, by which it can be held, and on the rest of the back are stamped, in relief, certain circles with a kind of Grecian border. Its polished face has that degree of convexity which gives an image of the face half its natural size; and its remarkable property is, *that when you reflect the rays of the sun from the polished surface, the image of the ornamental border, and circles stamped upon the back, is seen* (we presume in shadow) *distinctly reflected on the wall.*

The metal of which the mirror is made, appears to be what is called Chinese silver, a composition of tin and copper, like the metal for the specula of reflecting telescopes. The metal is very sonorous. The mirror has a rim of about $\frac{1}{4}$th or $\frac{1}{6}$th of an inch broad, and the inner part, upon which the figures are stamped, is considerably thinner.

Mr. Swinton states, that no person he has met with has either seen or heard of anything similar to this mirror. The gentleman who brought it from China, says that they are very uncommon in that country; and that this one, with a few others, was brought by a Dutch ship from Japan several years ago. On the back of one of these was a *dragon*, which was most distinctly reflected from the polished side. Mr. Swinton also mentions that he has seen another Chinese circular mirror, which is curiously embossed on the back. It is *eight* inches in diameter; but as its polish is rubbed off, he has not yet been able, by replacing it, to ascertain if it reflects a picture similar to the figures stamped upon its back. Mr. Swinton adds, that the original mirror first described, is to be

sent to England, either to Sir John Herschel, or to the writer of this notice; and in the mean time he proposes to us the question, "How are these strange optical effects produced?"

Mr. Swinton himself ingeniously conjectures that the phænomena may have their origin in a difference of density in different parts of the metal, occasioned by the stamping of the figures on the back, the light being reflected more or less strongly from parts that have been more or less compressed. If metals were absolutely opaque, and if the light which they reflect never entered their substance, as in the case of reflexions from transparent bodies, then the only possible way by which they could give a picture of the figures stamped behind would be that which Mr. Swinton suggests *.

I believe, however, on the authority of the phænomena of elliptical polarization, that in silver nearly one half of the reflected light has entered the metal, and in other metals a less portion; so that we may consider the surface of every metal as transparent to a certain depth,—a fact which is proved also by the transparency of gold and silver leaf. Now this thin film having its parts of variable density in consequence of the stamping of the figure, might reproduce the figure by reflexion. It is well known that silver *polished by hammering*, acts differently upon light from silver that has received a *specular polish*; and I have elsewhere† expressed the opinion that a parabolic reflector of silvered copper polished by hammering, will, from the difference of density of different parts of the reflecting film, produce at the distance of many miles a perceptible scattering of the reflected rays similar to what takes place in a transparent fluid or solid, or gaseous medium. I am satisfied, however, that, at the distance of a few inches from the Chinese mirror, this evanescent effect will be altogether imperceptible, and that we must seek for another cause of the phænomenon under consideration.

Some years ago I had occasion to observe the light of the sun reflected upon paper from a new and highly-polished gilt button, and I made a drawing at the time of the figure which appeared in the spectrum. It consisted of radiations exactly like the spokes of a carriage-wheel, the radiations being *sixteen*

* A series of very pretty deceptions might be made on the same principle, by painting (with thin transparent varnishes laid on in narrow lines) a figure on the back surface of a plate of glass. The figure would be seen by reflecting the light of the sun upon a wall, in consequence of the reflexion being destroyed, or nearly so, at those parts of the back surface which are covered with the varnish, and of the light being scattered at the outer surface of the varnish. In ordinary lights the lines would not be visible, but they would distinctly appear in the reflected rays of the sun.

† Edinb. Trans. vol. xi. p. 47.

in number, and a little confused in the centre opposite the eye of the button. On the back of this button several words were deeply stamped, but these words did not appear in the reflected image. I have since examined several varieties of such buttons, and I find that they almost all give either radiations or great numbers of narrow concentric rings, (and sometimes both), whose centre is the centre of the button, and the smallest one of which is always like a dimple in the centre.

Upon examining the surface of these buttons in the sun's light and at the edge of a shadow*, I have invariably been able to see the same rings excavated in the polished face that appeared in the luminous image, which it reflected. They obviously arise from the button being finished in a turning lathe, and the rings are produced by the action of the polishing powder, or probably, in some cases, they may be the grooves of the turning tool, which have not been obliterated by the subsequent processes†.

These facts will, I presume, furnish us with the secret of the Chinese mirror. Like all other conjurors, the artist has contrived to make the observer deceive himself. The stamped figures on the back are used for this purpose. The spectrum in the luminous area *is not an image of the figures on the back*. The figures are a copy of the picture which the artist *has drawn on the face of the mirror*, and so concealed by polishing, that it is invisible in ordinary lights, and can be brought out only in the sun's rays.

Let it be required, for example, to produce the dragon described by Mr. Swinton, as exhibited by one of the Chinese mirrors. When the surface of the mirror is ready for polishing, the figure of the dragon may be delineated upon it in extremely shallow lines, or it may be eaten out by an acid much diluted, so as to remove the smallest possible portion of the metal. The surface must then be highly polished, not upon pitch, like glass and specula, because this would polish away the figure, but upon cloth, in the way that lenses are sometimes polished. In this way the sunk part of the shallow lines will be as highly polished as the rest, and the figure will only be visible in very strong lights by reflecting the sun's rays from the metallic surface.

When the space occupied by the figure is covered by lines or by etching, the figure will appear *in shade* on the wall, but

* By this method the figure in the Chinese mirror could be rendered visible beneath its polish.

† In polished steel buttons the reflected light is crowded with lines running at right angles to each other, and clearly indicating the cross strokes by which they have been ground and polished.

if this space is left untouched, and the parts round it be covered by lines or etching, the figure will appear most luminous.

We would recommend this subject to the notice of the optician, as likely to furnish him with a lucrative article of trade.

Allerly, Nov. 8, 1832.

LXXIX. *Notice on the Chemical Action of the Magneto-electric Currents. By Professor* BOTTO, *of Turin**.

AMONGST the characters which it is important to determine with reference to the knowledge of the nature of the magneto-electric currents discovered by Faraday, is that of their *chemical action.* As decisive of this point, I will state the results I have recently obtained, limiting myself for the present to a mere announcement; since they make part of other results belonging to a series of careful experimental inquiries, intended to clear up certain points of the theory of electro-magnetism, which will be published in due time.

The apparatus which I used to examine the chemical efficacy of the Faradian currents [*Faradiane correnti*] consisted principally of an artificial horse-shoe magnet, and a bar of soft iron surrounded in the middle by a magneto-electric spiral. The extremities of such a bar may, by help of a very simple arrangement, be separated at will from the poles of the magnet, and restored again to their first position with any required degree of rapidity.

The apparatus is inclosed within a wooden box, and is put into activity by an external handle. The box is surmounted by two rods, so connected (moveably) with the internal mechanism as by means of it to interrupt or re-establish the current, at pleasure, and at the moment most favourable to the production of a spark. When the spark is to be obtained, it is only necessary to connect these rods with the extremities of the magneto-electric spiral. When the apparatus is to be adjusted for chemical decomposition, the extremities are to be otherwise arranged, and so that the substance to be decomposed enters into the circuit.

Water, sulphate of copper, acetate of lead, and other salts in solution were thus submitted to trial. At first, minute quantities of the substances were acted upon, because of the feeble power of the apparatus (the magnet scarcely lifting six pounds, Piedmontese), and the presumed relative tenuity of the current: but I was not long in ascertaining that such was

* Communicated by the Author.

the energy of the latter, that I could act with success on larger quantities.

Two platinum wires as conductors were fixed by cement in two holes made in the side of a small glass; the latter was then filled with water, made a better conductor by a few drops of solution of soda, and inverted in a vessel filled with the same fluid. The communication was then completed, between the platinum conductors, and the extremities of the rods connected with the magneto-electric spiral, and the apparatus was worked. So soon as the successive detaching and attaching action began, the divellent forces of the platinum poles became evident, and an infinity of gaseous bubbles rose from them in the form and appearance of two columns of vapour: in a short time these being collected in the top of the glass, produced a portion of oxygen and hydrogen capable of causing a sensible detonation.

The phænomena became even more interesting, when the evolution of the two gases was observed through a powerful lens; the bubbles succeeding each other the more vigorously as the alternate action of the magneto-electrometer is more rapid. My colleague Professor Michelotte, to whom I communicated these results, wished me to produce them at the Cabinet of Philosophy in the University, where the experiment was repeated under his own eyes.

I shall refrain from describing at present other results obtained by attempting the solutions of various metallic salts: in general the analogy between the effects produced and those of the hydro-electric currents appeared perfect; at least when due regard was given to the continuity of these, and to the intermitting and fugacious nature of the magneto-electric currents;—to the constant direction of the former, and the alternate opposition of the latter. At present it is not easy to predict by what the means of exciting and increasing the chemical efficacy of the magneto-electric powers will be limited; but it is certain that such a character, highly interesting as it is in the philosophy of imponderable agents, deserves to fix the attention of men of science.

Turin, Oct. 12, 1832.

LXXX. *Notes on the History of English Geology.* *By* William Henry Fitton, *M.D. F.R.S. &c.*

[Continued from p. 275.]

In this enumeration of authors, which we have now brought down to the period when Geology, as a branch of inductive science, may be said to have had its birth in England, we have

omitted to notice the greater number of an extensive class of writers, the older British Topographers; who contributed to the progress of the subject rather by supplying detached facts and local information, than by connected views of the structure of the country: and hence, although their works may be consulted with advantage by those who are employed in investigating local details, they do not claim particular notice, where general principles are the chief objects of inquiry.

There is one, however, among the topographic antiquaries, who ought, perhaps, to have been mentioned at the very commencement of this paper; and who deserves to be had in remembrance, as the Patriarch of English Geologists;—though his work remained unknown for more than two hundred years. George Owen of Henllys in Pembrokeshire, Lord of Cemaes or Kemes, was the author of a History of that county, the manuscript of which bears the date of 1595, during the reign of Elizabeth;—perhaps more than a century before any thought of tracing the strata of England or of the globe, had been acted upon, or even mentioned in this country: but the work remained in manuscript till 1799, when it was published in the Cambrian Register*. The author enters largely and with great intelligence, into topographical and statistical detail; and in one of his chapters, treating of the 'natural helpes, 'which is in the countrey to better the lande'—of which he reckons 'lyme' to be the 'chiefest,'—'First,' he says, 'you 'shall understand, that the lymestone is a vayne of stones 'running his course, for the most part right east and west, 'although sometimes the same is found to approach to the 'north and south.— Of this lymestone there is found of an'cient, two veynes, the one small and of no great account; 'and not of bredth above a butt length, or stones cast; and 'therefore whosoever seeketh southward or northward over the 'bredth misseth it.'—The course of this 'veyne' is then traced to a considerable distance eastward, out of Pembrokeshire. 'The other vayne of limestone, and chiefest of the two, is about 'seven miles distant from the former, more southerly then it, 'and soe or neare they continue together as shall be de'clared;'—and its course is in like manner traced towards the east, to where 'it taketh water,' and passing under the sea,— 'as reason and the course thereof leadeth us to think,' is again

* 'A History of Pembrokeshire, from a manuscript of George Owen, 'Esq. of Henllys, Lord of Kemes, &c.—now first published by his great'grandson Richard Fenton, Esq.' Cambrian Register for 1796, vol. i. p. 52. London, 1799.—An extract from this History, containing what relates to Coal, has been printed also in Fenton's Historical Tour through Pembrokeshire,—Appendix, p. 54.

resumed in the land;—and it is followed in detail to Chepstowe, and for some distance beyond that place.

'This digression,' he adds, 'concerning these two vaynes 'of limestone, taking their original here in Pembrokeshire, 'I have thought good to insert in this place; for at the re-'quest of a dear friend of myne, and famous for his learning, 'I took some paynes about it,—finding the natural course 'thereof to be as before a thing perchance not so well noticed 'as fitt to be known; and being noted and knowne, it may be 'a guide to some parties to seek the lymestone where it yet 'lyeth hidd, and may save labour to others in seeking it, 'where there is no possibility to finde it.'

A third 'veyne of lymestone,' is also noticed, more northerly than the other two,—(probably one of the subordinate beds of the transition series),—which is correctly distinguished from those above mentioned, and likewise traced, as far as the author's acquaintance with the country extended.

'For the veyne of coales—which is found between these 'two vaynes of lymestone, as a benefit of Nature, without 'which the profit of the lymestone were neare lost:—betweene 'the sayd two vaynes from the beginning to the ending, 'there is a vayne (if not several vaynes) of coles, that fol-'loweth those of the lymestone, — This vayne of cole in 'some partes joineth close to the first lymestone vayne, as in 'Pembrokeshire, and Carmarthenshire; and in some partes 'it is found close by the other vayne of lymestone, as in Gla-'morgan, Monmouth, and Somersetshires. Therefore,' it is 'cautiously added, 'whether I shall say that there are two 'vaynes of coles to be found betweene these two vaynes of 'lymestone, or to imagine that the cole should wreathe or 'turne itself, in some places to one, in other places to the 'other; or to think that all the land betweene these two vaynes 'should be stored with coles,—I leave to the judgement of 'the skilfull miners, or to those which with deep knowledge 'have entered into these hidden secrettes.'

Now these 'two vaynes of lymestone' are in fact the boundaries, on the north and south, of the great coal-tract of South Wales; and if the reader will compare Mr. Owen's descriptions, or even our brief abstract, with a good map, and with the account of that tract since published by Mr. Martin *, he will be surprised, perhaps, at the coincidence, and will regret that a work so valuable remained so long unknown and unproductive;—since it would be difficult to produce, even at the present time, a better specimen of geological investigation.

* Philos. Trans. 1806, vol. xcvi. p. 342.

Another class of authors, still less deserving of specific notice than the topographers, and fortunately of much more frequent appearance during the seventeenth and the beginning of the last centuries, than of later years, is composed of those who mingled Scriptural history with speculative or ideal geology, and weakly fancied that they maintained the authority of the Scriptures, and promoted the cause of truth,—by seeking for traces of the Deluge in all the appearances of the earth, and warping into accordance with the Mosaic account of the Creation, their own scanty and inaccurate notions of the structure of the globe. Of these writers the greater number appear to have forgotten the danger which attended their presumptuous attempts; since if they had succeeded in establishing the connexion of their own views with the sacred writings, the fall of their opinions (and, one after another, they have all passed away,) must necessarily have been accompanied by that of Scriptural authority. An instance of the ill effects of this mode of proceeding has been already noticed, in the history of organic remains; and it would be easy to multiply quotations, which might perhaps surprise our readers of the present day*. There are, however, some very honourable exceptions to these general remarks. The works more especially of Catcott†, in the last century, and more recently of Mr. Townsend‡, whom we have already mentioned, afford in many instances correct views of the operations of nature, and valuable statements of fact; notwithstanding their erroneous notions, as to the objects of geology, and the mode of conducting inquiry in this as in every other department of scientific research.

In that part of Mr. Catcott's work which goes to demonstrate, to use the language of Cuvier, 'that the earth has been 'recently overwhelmed by the waters of a transient deluge,' there are many excellent observations: but in attempting to include the solid strata within the range of that operation, and ascribing to it the presence of the fossils which they contain, the author shares the fate of all those who before him had indulged in similar speculations: and his Diagram,—'representing the internal structure of the terraqueous globe, from 'the centre to the circumference,' is the result of suppositions not less visionary than those of Burnett, Hutchinson, and

* Some further reference to the writings alluded to in the text, will be found in an article by the author of the present paper, in the Edinburgh Review of Dr. Buckland's '*Reliquiæ Diluvianæ*:' Edin. Rev., October 1823, vol. xxxix. p. 196.

† Catcott, 'A Treatise on the Deluge.' 8vo, London, 1761.

‡ Townsend, 'Vindication of Moses,' &c. 1813.

other writers, who treat professedly of events beyond the limits of human observation*.

Having thus sketched the progress of facts and opinions, to the period when Mr. Smith began his researches on the stratification of England, we shall next inquire respecting the method of expressing the results of geological observation by means of maps.

We have seen that LISTER, though he did not carry into execution his own 'project for a Map of Soiles,' entertained a very philosophic expectation of the benefit that might result from it:—'If the limits of each soil,' he says, 'appear-'ed upon a map, something more might be comprehended 'from the whole, and from every part, than I can possibly 'foresee; but I leave this to the industry of future times†.' We now know how amply the advantages to science, foreseen by the author of this project, have been realized. A still more refined, and, as it then may have appeared, more remote anticipation of the future progress of geological inquiry, occurs at the close of FONTENELLE's observations on a paper of De Reaumur, giving an account of a remarkable accumulation of fossil shells in Touraine:—'M. de Reaumur 'imagine comment le Golfe de Touraine tenoit à l'océan, et 'quel étoit le courant qui y charioit les coquilles; mais ce 'n'est qu'une simple conjecture, formée pour tenir lieu du 'véritable fait inconnu, qui sera toujour quelque chose d'ap-'prochant. Pour parler surement sur cette matière, *il fau-'droit avoir des espèces de Cartes Géographiques dressées selon 'toutes les minières de coquillages enfouis en terre. Quelle 'quantité d'observations ne faudroit il pas, et quel temps, pour 'les avoir! Qui scait cependant, si les sciences n'iront pas un 'jour jusque-la, du moins en partie* ‡?'

It is now little more than a century since this passage was written: yet, if geology advances during the next hundred years as it has done during the last fifty, is it not highly probable that the prophetic anticipation of Fontenelle will have been fulfilled?

* The title of Burnett's eloquent and celebrated work, '*Theoria Sacra,*' is as follows: 'The Theory of the Earth, containing an account of the 'original of the earth, and of all the great changes which it hath already 'undergone, or is to undergo, till the consummation of all things.' 3rd edition: London, 1697.

† Lister, Phil. Trans. vol. xiv. p. 739, &c.

‡ *Histoire de l'Academie Royale des Sciences*, 1720, p. 5; and *Mémoires*, p. 400.—The report here referred to is ascribed to Fontenelle on the authority of Faujas. *Œuvres de Palissy*, 4to, p. li.

About fifty years after Lister's project for a geological map, a work was published, under the title of 'A new Philosophico-'chorographical Chart of East Kent, invented and delineated 'by CHRISTOPHER PACKE, M.D.,' which is one of the most valuable contributions to the physical geography of England that has appeared. It was preceded by a letter to the Royal Society, and accompanied by a Tract of greater length*, explaining the purpose of the work; which, from the author's frequent employment of anatomical terms, seems to have been suggested by his professional studies. The map itself represents, on a scale of rather more than an inch and half to a mile, a circle of about two and thirty miles round Canterbury: the principal object being, as the title of the Tract imports, to represent the course and connexions of the valleys, all of which are described with great minuteness of detail,—their ramifications being compared to those of the veins in the human body. As the greater number of these valleys are at present without streamlets, it is inferred that they were formed not by existing causes, but by the retiring waters of the Deluge; and that the surface since that event has undergone no change. There is no allusion to stratification either in the map or the memoir; but the natural features of the country are very correctly distinguished, and divisions pointed out, which correspond with those of the present day:—the first including the chalk district; the second, under the name of 'stone hills,' the ridge of the lower greensand;—between which and the chalk range, the vale occupied by the gault is also clearly indicated:—and the 'clay-hills,' constituting a third division, occupy the valley of the Weald. Nothing can be better than the general plan upon which the author proceeded; more perfect execution only, having been wanting to render his map complete: and he seems himself to have had a just sense both of the importance of his undertaking, and of the true mode of accomplishing it.—'For this,' he says, 'is no dream or devise, the offspring of a sportive 'or enthusiastical imagination, conceived and produced for

* The title is 'ΑΓΚΟΓΡΑΦΙΑ, sive Convallium Descriptio; in which 'are briefly, but fully, expounded the Origine, Cause, and Insertion, Extent, Elevation, and Congruity, of all the Valleys and Hills, Brooks and 'Rivers; as an Explanation of a new Philosophico-chorographical Chart of 'East Kent. Canterbury, 1743.'

This Tract, which everywhere shows the patriotism of the author, and his enthusiasm about his subject, contains some very amusing passages. He rejects indignantly the title of 'Map' for his performance; 'there 'being,' he asserts, 'as manifest a difference between this chart and a map, 'as there is between the frame of any building, and the same finished into 'a complete house, adorned with all its furniture.'

'want of something else to do, at my leisure in my study,—but 'it is a *real* scheme, taken upon the spot with patience and 'diligence, by frequent or rather continual observations, in 'the course of my journeys of business through almost every 'the minutest parcel of the country: digested at home with 'much consideration, and composed with as much accuracy, 'as the observer was capable of.'—P. 98.

Buache's map of the Northern Hemisphere, published in 1756*, with his other productions, relate more properly to physical geography, than to geology; and were founded upon an hypothesis which assumed the existence of a frame-work, or skeleton, of the earth, consisting of chains of mountains,—which were supposed to traverse not only the continents, but the seas and oceans, throughout the face of the globe. The islands were considered only as the more prominent points of these chains; and in order to connect the islands of the greater oceans with the continents, the author was obliged to form by interpolation, or to imagine, submarine chains, of many thousand miles in length.

Guettard appears to have been the first author, in France, who formed the project of a mineralogical map. His plan was that of representing upon ordinary maps, by means of detached characters, the several mineral substances found at each point observed: but his general views were very loose and hypothetical; nor was there a sufficient stock of facts, at that time, to support them.

The Mineralogical Atlas and Description of France, by Monnet†, was undertaken and conducted in continuance of Guettard's, and expressly upon his principles; though, for some reasons which are not stated, he himself withdrew from the direction of the work. It was an elaborate undertaking; and the value certainly is not proportioned to the labour and expense bestowed upon it: though, if the observations were correct, the collection would still furnish useful materials to those who examine the country with sounder general views. The great defect appears to be, that the authors of the work do not seem to have been impressed with, or to have acted upon, the stratigraphic principles, so well explained by Michell

* Philippe Buache,—*Essai de Geographie Physique, où l'on propose des vues generales, sur l'espèce de charpente du globe, composée de chaines des montaines, qui traversent les mers comme les terres.*—Mem. de l'Acad. 1752, pp. 399, 416.—Buache was born in 1700, and died in 1773.

† *Atlas et Description Minéralogique de la France, entrepris par ordre du Roi; par MM. Guettard et Monnet. Publiés par M. Monnet, d'après ses nouveaux Voyages—1re Partie;*—Paris: folio, 1780; pp. 212, with 31 Maps.

twenty years before its publication; and this is the more extraordinary, as vertical sections, detailing the order of the beds, accompany the maps. Some of the copies, which we have seen,—in which the characters expressing the predominant mineral substances are coloured,—approach so near to the expressive power of the modern geological maps of stratified countries, that one is surprised that the authors did not, by advancing this step, give that connexion to their results, which is the essence of geology.

It is not a little remarkable that De Saussure, who published some years after the appearance of Monnet's atlas, and must have been acquainted with that work, as well as with the maps of the German school*, does not appear to have attempted any geological map of the tracts he has described. Had he made that trial, it is probable that he would have anticipated some of the important results which have since been afforded by the maps and sections of the Alps, by Ebel and Escher†.

We have not yet seen the maps referred to by the late M. Desmarest, as intended to be annexed to the *Encyclopédie Méthodique;* but this writer judiciously insists on the benefit arising even from attempts to express in maps the results of geological investigation; and on the advantage of combining with them vertical sections of the tracts represented.

Some of the geological maps in colours, of the older German mineralogists, are valuable; but the best plan,—which Mr. Jameson has informed us, was devised, or much improved, by Werner‡,—is that of representing the several formations in distinct but sober hues, and marking the superior rock by a narrow band of deeper colour along the line of its contact with the subjacent one; and this is nearly the method which is employed by English geologists at present.

Of the county surveys, published by the Board of Agriculture in 1794, five only have maps which indicate the composition of the surface; and of these, that of Devonshire alone has any pretension to geological distinction,—enumerating dunstone and limestone in its list of '*Soil.*'—The remaining four, Lancashire, Lincolnshire, Sussex, and Wiltshire, represent by colours the superficial *soils*, in the agricultural sense of the term;—the first two distinguishing also the *coal* tracts. But

* The four volumes (4th edition) of De Saussure's *Voyages dans les Alpes,* are dated respectively,—I. 1779; II. 1786; III. and IV. 1796.

† *Uber der Bau der Erde in dem Alpen Gebirge.* II. Tom. 8vo. Zürich, 1808.

‡ Transactions of the Wernerian Society, vol. i. p. 149.

there is not in any of these maps any intimation of stratigraphical structure, nor are any sections connected with them.

We are not, indeed, aware that any maps which can be called geological, had appeared in this country before that of Mr. Smith; unless two of the Plates which accompany the Historical Atlas of England, by ANDREWS, published in 1797*, are entitled to that name. These, however, though very defective in execution, and on a very small scale are at least, well intended: one, exhibiting the basins, valleys and courses of the great rivers; another being entitled, 'A map of the summits of the chain of mountains and great ridges of hills of '*Albion*, as it is supposed they appeared when the water was 'descended after the Deluge.' This last plate is a sort of skeleton of England, after the manner of Buache, whose system the author seems to have imbibed; it points out the great ridge of "yellow limestone" (perhaps the oolite), and the chalk ranges,—from Sidmouth to the sea in Norfolk, and eastward through Surrey, Kent, and Sussex. To a person acquainted with the geology of England, such a sketch presents some interesting views but nothing respecting stratification, nor the internal structure of the country, is either indicated in the maps, or mentioned by the author in his treatise.

[To be continued.]

LXXXI. *Notices respecting New Books.*

Dr. PEARSON'S *Introduction to Practical Astronomy.* 4to. 2 vols.

[Continued from p. 375.]

THE first volume of this laborious publication consists of a complete series of astronomical Tables, arranged in the most convenient form for giving the corrections. In the completion of this arrangement, the author has evidently spared no trouble or expense; and he has availed himself of the advice and labours of other computers. In arranging the series of *general* Tables of Precession, Aberration, Lunar and Solar Nutation, where much care is requisite, we are glad that his own labours were assisted by the mathematical talents of the late Cape Astronomer, Mr. Fallows, who was remarkable for his great skill and precision in such arrangements, and to whom the author has expressed a sense of great obligation. With such helps, and by his own indefatigable industry, our author has produced a collection of Tables, which are among the most extensive and valuable that have ever appeared.

* "Histoircal Atlas of England, physical, political, astronomical, &c. from the Deluge to the present time; by John Andrews, Geographer," &c. London, folio, 1797; printed for the author. This work does not appear to have been completed. The maps are only 13 inches in length by 10 in width.

This volume is dedicated to the Astronomical Society of London, of which Dr. Pearson was for ten years the treasurer; and he has taken this opportunity of showing his high sense of the honour conferred upon him, by dedicating to its members in general this collection of Tables, so well calculated to promote the objects for which the Society was instituted.

"Some opinion may be formed of the extent of the author's labours, when it is stated, that, of the 457 pages constituting this volume, 325 contain new Tables, or explanatory matter; 46 are filled with Tables that have been enlarged, or otherwise improved; and 86 only comprise Tables that have been copied in their original state."

The first set of Tables contains the corrections to be applied to the apparent place of a star, in order to obtain its place clear from the effects of *refraction*.

The first four Tables are computed from the formula of Bradley; and as the explanation given of their use is quite clear, we need say no more about them. The Tables of Refraction next in order are those published in 1806 by the French Board of Longitude: these, as our author informs us, are founded on the profound investigations of La Place. But whether the most elaborate formula deduced by this eminent geometrician is more to be depended on than that given by Bradley, may perhaps be questioned. The arrangement given by our author is that adopted at Greenwich. The next set of Tables for this purpose are those computed by Stephen Groombridge, Esq., who conducted a long series of observations for this purpose, with one of the most perfect meridian instruments that was ever constructed. We allude to the beautiful *transit circle* made by Troughton, and which is fully described by our author in his second volume.—The Tables next in orde rare Piazzi's: these were constructed without any reference to theory, and were deduced from observations made out of the meridian.

Among those who have laboured in this interesting department of science, the late Dublin astronomer, Dr. Brinkley, must not be passed over. Our author has introduced the Tables computed by this acute philosopher, and they deserve notice, inasmuch as the formula on which they depend is original, and displays the greatest ingenuity. We refer our readers to the original papers in the Irish Transactions, as they are most peculiarly interesting and instructive. We also refer to the account and explanation of these Tables given by our author.—The late Dr. Young, whose penetration was so remarkable that few things passed under his observation without undergoing some improvement, also investigated a formula for refraction. Our author informs us that the Tables computed from this formula are said to agree more exactly with the latest observations than any others.

The Tables following are constructed by the indefatigable Bessel. Our author's account of them is as follows: "He has investigated the subject of Refraction, and, retaining the characters of La Place, has deduced the following formula, on which he calcu-

lated a set of Tables that comprehend quantities even too minute to be generally serviceable in practical astronomy."

The addition of Bessel's Tables, as also of Carlini's, with the Tables computed from the elaborate formula of Mr. Ivory; also of Littrow's and Zach's modifications of Bessel's Tables,—seems to have been an after-thought on the part of our author: yet in doing this he has adhered to his original determination of sparing no trouble in making his collection as complete as possible.

We come next to another set of the most important Tables. These are the Tables from I. to XIV. inclusive, originally proposed by Baron Zach, and reduced to their present shape by the late Mr. Fallows. They are arranged in two sets, the first containing the corrections in right ascension, and the rest the corrections in north polar distance for precession, aberration, lunar nutation, and solar nutation.

Table XV. contains the proportional parts of the annual precession, so as to give the correction for any given day of the year.—Tables XVI. and XVII. are for the purpose of giving the sun's longitude correctly at the *moment* of the culmination of any known star; for as the aberration and solar nutation are both functions of the sun's longitude, neither will be obtained correctly unless this longitude be accurately known at the instant of observation. Table XVI. gives this; and Table XVII. gives it also with still greater accuracy. But this last Table also answers another purpose, viz. that of adapting the sun's longitude, calculated for one observatory, to the meridian of any other.

The labour bestowed on Table XVIII. must have been very considerable. We will refer to the author's own account of this Table, given in page 322, and so leave it with our scientific readers to appreciate its merits. We need only observe, that although to an "expert mathematician" there would be no *difficulty* in computing these numbers, yet having them computed for him, saves him a great deal of trouble; and therefore it is not only to those "who are not skilled in trigonometrical calculations" that this Table is useful, but to every practical astronomer. This Table consists of 45 pages; and we will readily take our author's word for it, that they "have been calculated at the expense of considerable labour."

The next Tables in the collection are the universal Tables of Delambre, which, from the facility with which they can be applied to single observations, our author has thought proper to introduce.

After these follow a few small Tables, called *Differential* Tables, the object of which is to give by inspection the changes which take place in the aberration and lunar nutation, when a small alteration, such as one degree, is given to the place of a star in right ascension or declination; and also two other Tables, the former for correcting the diurnal aberration, and the latter for determining the aberration of light in the case of a comet or planet.

The next series of Tables is of a different kind, inasmuch as the corrections to be taken from them do not result from physical

causes. The first of this series is for the purpose of reducing observations made out of the plane of the meridian, to what they would have been had they been made in that plane. This method of observing is particularly useful with the altitude and azimuth circle. There are two Tables given for this purpose: one by Delambre, and another somewhat more simple by the late Dr. Young.

For an account of the Tables entitled "Terrestrial Graduation," we refer to the detail given by our author.

The Tables for converting space into time, and the contrary, will be found very useful on many occasions. Also the new Tables for converting solar into sidereal time and the converse, with one for obtaining the proportional parts of the clock's daily rate, will be found equally useful.

We now come to a set of Tables of the corrections to be applied to any one of the forty-eight stars for which they are calculated. The object of these Tables is to obtain the mean place, the amount of the several corrections, and the true apparent place of any one of these forty-eight stars, for any day of the year for many years to come.

This set of Tables is followed by another containing the comparative mean places of the same forty-eight principal stars, as recently determined by the most eminent astronomers for the epoch 1800.

The Table in page 147 is useful as containing the changes which take place in the arguments, with which the above Tables are entered, depending on the value of the subsidiary angle used in their computation. By means of this Table the corrections for the forty-eight principal stars may be adapted to any year for a long period before or after the year 1830.

The Table next in order is that of Bessel for determining by an abridged method the apparent right ascensions of Dr. Maskelyne's thirty-six stars.

We now arrive at another class of Tables, viz. Solar Tables, of which there are twenty-five, contained from page 153 to 180 inclusive. Table I. contains the mean longitudes of the sun and of his perigee for one hundred and fifty years, commencing with the year 1750. Table II. is supplementary to Table I., and contains the quantity to be added to the mean longitude of the sun, and of his perigee, to obtain the mean longitude for *any day* in the above-mentioned period. Of Tables III. and IV., the first is for the purpose of obtaining a near approximate value of the sun's *true* longitude, as the argument with which some of the preceding Tables are entered; and the last for obtaining an approximation to the sun's *mean* right ascension, for the purpose of comparing solar with sidereal time. Table V. affords the means of obtaining the sun's right ascension when his longitude is given. Table VI. consists of the sun's declination, corresponding to every 10′ of his longitude, with an equation for a small variation in the obliquity. The use of Table VII., containing proportional parts of the sun's daily variation in longitude, is obvious. Table VIII., entitled "Reduction

of the Ecliptic to the Equator," is only a modification of Table V., the *difference* between the sun's longitude and right ascension being given in space. Table X., entitled "Reduction to either Solstice," furnishes the means of obtaining the sun's declination at the solstice, (*i. e.* the apparent obliquity of the ecliptic,) from observations of his right ascension and declination when *near* the solstice. We refer our readers to this, and Table XX., with our author's explanation of both, and also to his explanation of the discrepancy existing between the winter and the summer reductions to the solstice, according to the Greenwich observations. Tables XI. and XII. contain minute equations of the equinox and obliquity depending on the *small* oscillations of the earth from the mean plane of her orbit, produced by the combined disturbances of the Moon, Venus, and Jupiter. The Tables XIII. to XIX. are all useful, and for their application we refer to the explanation given in the work.—When the time of apparent noon is determined by means of equal altitudes of the sun, a small equation is found requisite on account of the change in the sun's declination in the interval of the observations. The Tables XXI., XXII. by Delambre, and XXIII., XXIV. by Zach, contain the corrections to be applied to obtain the true time of the passage of the sun's centre over the meridian. These Tables are essential when a sextant or any equal altitude instrument is made use of, but are superseded by the use of the transit instrument. Table XXV. being the last of the Solar Tables, contains the sun's parallax in altitude: and the method of taking this from the Table, when the horizontal parallax is given, is obvious.

The Tables next in order are the Lunar Tables. There are fifteen of these contained in pages 181 to 203 inclusive. Table I. contains the epochs of the mean longitude of the moon's ascending node, for 120 years, beginning with the year 1800; and Table II. contains the proportional parts of the annual regression to be applied to the mean longitude of any epoch, to obtain the mean longitude for any day of the corresponding year. Table III. gives the moon's semidiameter for every second of her equatorial horizontal parallax. Table IV. contains the differences of the moon's horizontal parallax in the sphere and spheroid computed for two different compressions. For the use of Table V., and also that of Table VI., containing the angles between the normal and the radius, in different parallels, we refer to our author. Table VIII. gives the moon's parallax in altitude, and is very conveniently arranged. Table IX. gives the time of the moon's semidiameter passing the meridian, and is of use in determining the moment of the passage of the moon's centre over the meridian: the example given by our author fully explains its use. Table X. is also for the same purpose.

The changes which take place in the moon's right ascension, declination, &c. being far from uniform, we cannot interpolate by means of parts taken proportional to the time elapsed, since she was in any given position: we must, therefore, in order to obtain a tolerable approximation, make use of the second differences. Table XI. is constructed for this purpose. Tables XII. and XIII. are both of considerable use for the practical purposes for which they are con-

structed. Tables XIV. and XV., giving the aspects, are of but little use; they are, as our author observes, "more calculated to gratify curiosity than to answer any useful purpose."

The next set of Tables are termed Zodaical Tables, so called by the author because they are applicable to those phænomena which occur in the zodiac, such as eclipses, occultations, &c. On this account some Tables which, strictly speaking, ought to have appeared among the Lunar Tables, find their place here; such as the Tables from XIII. to XXV. inclusive, relating to the moon's parallax in longitude and latitude, in right ascension and declination. These Zodiacal Tables extend from page 203 to page 261. The first is the converse of the Solar Table V. or VIII., and is given for the purpose of obtaining the sun's longitude from his right ascension. Tables II. and III. are very laborious Tables, originally constructed by Dr. Maskelyne, but here enlarged and improved by our author. Tables IV. and V. contain corrections to be applied to the quantities determined by the preceding Tables, for a small variation in the obliquity. As one of the principal uses of these Zodiacal Tables is to furnish the elements of computation in the determination of the longitude from an occultation of the fixed stars, it is of importance to know what stars may be occulted by the moon. Table VI. is introduced for this purpose, as copied from the *Jahrbuch* of 1780; but an enlarged one, in the order of right ascension, is subsequently given in the Appendix (p. 515—528).

As the longitude and altitude of the nonagesimal, or the highest point of the ecliptic above the horizon, are requisite elements in the determination of the moon's parallax in longitude and latitude, a Table for facilitating these laborious computations by furnishing the above elements must be of great use. For this purpose Table VII. has been constructed. The angle of position is also another useful element on many occasions: the two Tables VIII. and IX. have been added by our author for facilitating its computation for zodiacal stars. This angle, with the latitude and longitude, enters into the formulæ for aberration, and is moreover an element in the computation of eclipses, parallaxes, occultations, &c. Tables XI. and XII. are only modifications of the Solar Tables VI. and V. Table XIII., with its appendix, is of more importance, as it furnishes us with an important element in the computation of occultations of the fixed stars by the moon.

In the explanatory part of the work we find a few additional Tables given; the first is for the correction of parallax in the spheroid, computed from a formula given by Dr. Young. Following this are six small Tables, the first of which gives the longitudes and latitudes of Dr. Maskelyne's thirty-five stars, and the rest relate to the corrections of the mean longitudes and latitudes.

After two Tables expressing the lengths of circular arcs in terms of radius, there follows in the proper order a collection of Planetary Tables. The first six of these are for determining the parallaxes of the planets. The seventh affords the means of converting the geocentric longitude of a planet into right ascension. Then follow five more

Tables, lettered from A to E; the first is a Table of Zach's, giving the horizontal parallax of each of the ten planets, and also their apparent diameters: the remaining four relate to the aberrations of the planets in latitude and longitude depending on the eccentricity of their respective orbits.

We apprehend that all the Tables of the planets are very far from being in a state of perfection; and we are glad to find that the Plumian Professor of the University of Cambridge, who fills the important and distinguished office of principal astronomer in the magnificent observatory recently erected there, has expressed his determination of paying attention to this much neglected department of astronomy. Without making any further comments of our own on this subject, we will quote the very pertinent and just remarks made by the Plumian Professor, Mr. Airy, in his Preface to the first volume of the Cambridge Observations: "The part of astronomy which appeared to me to have been most neglected, at least in the observations of this country, is the observation of the planets. And the deficiency in this respect is most deplorable. In the published observations of our national establishment, there is not a sufficient number of observations of the planets to assist, in any material degree, in improving their theory. And any one who wished to revise the planetary Tables would now find himself nearly destitute of the necessary data upon which to found his investigations. As soon, therefore, as the Cambridge Observatory was placed under my direction, I determined to make the observation of the planets the leading object of my labours. And I was further led to do this by the consideration that my own personal exertions would probably be sufficient to accomplish the greatest part of this undertaking; but that, unassisted as I was, I could not hope to complete any plan of a more extensive nature." We most cordially wish this learned gentleman success to his labours in this much neglected field of science, the intricacies of which he, on account of his very great talents, is so peculiarly qualified to unravel.

Our author's next Table, headed *Velocity of Light*, is perfectly plain in its construction and use. Then come seven Tables relating to the mean places of the pole star; and we refer our readers to the work for their use and explanation. The next two Tables are of considerable use in adjusting the meridian position of the transit instrument. The first is a catalogue of circumpolar stars, to be observed both at their upper and lower culminations. Dr. Pearson has given another Table for the same purpose, to which we also refer, containing a collection of stars of nearly the same right ascension, but having declinations of opposite denominations. After these come a few Tables, for different purposes, with which the volume in its original form closes. Among these additional Tables we find Schumacher's catalogue and constants for 500 stars, and also the second series of Tables by the late Cape Astronomer.

With these Tables the work in its original form concludes.—Since its publication, however, the author has made a large addition to it

as an Appendix. The reasons which induced him to make this addition will be best understood by consulting the remarks at the beginning of this Appendix.

The author has also given a set of Tables for computing some very minute corrections depending on *twice* the longitude of the moon's ascending node (2. ☊). He also gives some account of Mr. Baily's Tables, computed for, and published by, the Astronomical Society of London.

Besides several other useful Tables, the author has added a catalogue of 520 zodiacal stars liable to occultation, containing the auxiliary constants for gaining the various corrections. This is accompanied by another table referring the same stars to the catalogue of the Astronomical Society. To these is added an arrangement of the above zodiacal stars in a table consisting of fourteen pages, before alluded to, in which the stars that may at a *given time* be seen occulted in England, are distinguished from those seen in any other country, giving also the place of the moon's node when the occultations may be expected.

A Guide to the Carpenter's Rule. By BENJAMIN BEVAN, Civil Engineer, London, 1832; 12mo. pp. 23.

We are informed in the preface to this pamphlet, that "the Author's mode of explaining the operations to be performed on the Slide-rule has been some years before the public" in his larger treatise on this instrument, and that it "is allowed to be superior to any other hitherto published." The present treatise is confined to those operations which can readily be performed with the *common Carpenter's Rule*, divested of the more extended formulæ contained in the larger one. For Schools, the author presumes, it "will be found useful in teaching instrumental arithmetic, and qualifying the student for the active pursuits of life." It consists of an Introduction, explaining the *notation* employed on the Rule, and of general formulæ, with examples, in the following arithmetical processes and branches of mensuration; viz.: Multiplication, Division, Proportion, Inverse Proportion, Squares and Roots, Cubes and Roots, Mensuration of Superficies, Solid Measure, Brickwork, and Gauging. These formulæ and examples are all perspicuously and accurately stated; and at p. 18, under the head "*To estimate the comparative Strength of Scantlings used in Buildings*," we find a very useful little table of the numbers expressing the comparative strength of various scantlings as used in different buildings.

LXXXII. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY.

THE following is a list of such papers read before, and communications made to, this Society, during the session of 1831—32, as have not already been noticed in our reports of its proceedings.

Jan. 13, 1832.—The following communications were read:—

I. The conclusion of Sir J. Herschel's paper on the Orbits of Binary Stars.

II. Account of the Occultation of 119 and 120 *Tauri* by the Moon, on December 18, 1831. By Sir James South. In a Letter to Mr. Baily.

III. Observations of Occultations of Stars by the Moon, made at Bedford, by Captain Smyth, between the months of January and December 1831.

IV. Observations of Occultations and of Stars observed with the Moon, at the Cambridge Observatory, during the year 1831. By Professor Airy.

V. Stars observed with the Moon at Greenwich, in December 1831. From the Astronomer Royal.

Mr. Baily announced from the Chair, that he had received a communication from Professor Schumacher, stating that His Majesty the King of Denmark had founded a gold medal, to be given to the first discoverer of a comet, not of known revolution, nor visible to the naked eye; subject to certain conditions, which have been recorded in the Phil. Mag. and Annals, N. S. vol. xi. p. 155.

(The proceedings of Feb. 10 will be found at p. 234, and those of March 9, at p. 390, of the present volume.)

April 13.—The following communications were read, &c.

I. Comparisons of the Right Ascension of Polaris, as obtained by observation, with that given in Bessel's catalogue for 1831; and observations of λ *Ursæ Minoris* and 51 (Hevelius) *Cephei*, made with a 5-feet transit at Blackheath. By Mr. Wrottesley.

II. Mr. Sheepshanks gave an account of the mural circle, with the methods of using it hitherto followed, and a description of the state and defects of the circle at the Cape Observatory.

III. A List of Stars observed with the Moon, at the Royal Observatory, Greenwich, March 1832.

IV. Transits of the Moon, with Moon-culminating Stars, observed at Cambridge in the month of May 1832.

Mr. Baily stated, in reference to Dr. Robinson's paper "On the dependence of a clock's rate on the height of the barometer," read in June 1831, of which an abstract was given in Phil. Mag. and Annals, N. S. vol. x. p. 445, that he had recently swung the mercurial pendulum in a vacuum apparatus, and found that a variation of one inch in the pressure of the atmosphere produced a difference of 0·42 seconds a day in the rate of the pendulum; which is more than double the quantity deduced by Dr. Robinson, from the observed alteration of rate, when compared with the height of the barometer.

May 11.—The following communications were read:—

I. On the transit of *Mercury*, which took place on the 5th instant: consisting of, 1. observations made by the Astronomer Royal and Mr. Simms, at the Royal Observatory; 2. by Mr. Riddle at Greenwich Hospital; 3. at the Observatory, Exchequer-street, Dublin; and 4. by Mr. Simms, in Fleet-street.

II. Observations of the Occultation of *Saturn* on the 8th instant:

1. at Greenwich Hospital, by Mr. Riddle; 2. at Islington, by Mr. Simms; 3. at Bedford, by Captain Smyth; 4. at Lapley, near Brewood, Staffordshire, by the Rev. M. Ward.

III. Observations of the Occultation of 119 and 120 *Tauri*, March 9, 1832, at Islington. By Mr. Simms.

IV. Correction of the Formulæ for determining the Longitude from an observed Occultation, with Suggestions for simplifying the application of these and other Formulæ to the case of corresponding Observations. By Mr. Riddle.

V. Observations of Eclipses of Jupiter's Satellites and Occultations between the years 1827 and 1832, and Measures of Double Stars taken in 1831 and 1832. Also Observations of the Comet of Ophiuchus in Jan. 1831. By the Rev. W. R. Dawes. Communicated by Sir J. F. W. Herschel.

VI. A New Method of reducing the apparent Distance of the Moon from a Star to the true. By Baron Zach.

VII. A Method of finding the rate of variation of the Moon's motion at any instant. By Lieut. Raper.

VIII. On the Determination of the Solar Parallax from Observations of the transit of Venus, June 3rd, 1769. A posthumous work of Don Joseph Joachim de Ferrar; followed by a Summary by Don Joseph Sanchez Cerquero. Communicated by Sir James South.

IX. An Ephemeris of Encke's Comet for the Southern Hemisphere. By Professor Encke. Communicated by Lieut. W. S. Stratford.

X. A List of Stars observed with the Moon, at the Royal Observatory, Greenwich, in the month of April, 1832.

XI. Observations of the Moon and Moon-culminating Stars made at Cambridge Observatory, in the month of April 1832; Long. $23^{s\cdot}$ 54 E., Lat. $52° 12' 50''$.

June 8.—The following communications were read:—

I. On the transit of *Mercury*, of the 5th of May 1832. By the Rev. J. Fisher. Communicated by Captain Beaufort.

II. Observations of the Occultation of Saturn of May 8, 1832: 1. by W. Lawson, and another at Hereford; 2. by Mr. Holehouse at Islington; and 3. by Mr. Snow.

III. Observations of the Solstices from December 1829 to December 1831; and Observations of Moon-culminating Stars during the year 1831; made at the Hon. East India Company's Observatory at St. Helena. By Lieut. Johnson. Communicated by Captain Beaufort.

IV. On the Calculation of the Geographical Longitude and Latitude of a place whose geodesic distance from the Meridian and Perpendicular of another place is given, the Earth being considered as a Spheroid. By Baron Zach.

V. Mean Right Ascension of Fifty-five unknown Stars, observed in 1831, with a transit instrument, of twenty inches focus, and a sidereal chronometer beating half-seconds; also Occultations observed in the month of March 1832. By Mr. Snow.

VI. Fifth Catalogue of Double Stars observed at Slough, in the years 1830 and 1831, with the twenty-feet reflector, and reduced to the epoch 1830·0. By Sir J. W. F. Herschel, K.G.H.

VII. Observed Occultation of Aldebaran. By Mr. Rothwell. Communicated by Mr. Tulley.

VIII. Ephemeris of Encke's Comet for the Southern Hemisphere. From Professor Encke.

IX. Stars observed with the Moon, at the Royal Observatory, Greenwich, in May 1832.

X. After the reading of the preceding communications, Mr. Sheepshanks made some remarks upon the mode of adjusting the transit instrument, and proceeded to describe minutely the various corrections requisite for obtaining correct results, illustrating his statement from various transit instruments furnished for the occasion by Messrs. Troughton and Simms.

ZOOLOGICAL SOCIETY.

Proceedings of the Committee of Science and Correspondence.

May 8.—A preparation was exhibited of the generative organs of a hybrid male bird, bred by the Society, and produced between a *Muscovy Drake* and a *common Duck*; and Mr. Yarrell described the external and internal appearances of the individual from which the preparation was obtained, and which, he stated, strongly resembled, externally, the true *Muscovy*, while internally the *viscera* were as decidedly indicative of the *common Duck*.

The skeletons of *Capromys Fournieri*, Desm., and *Dasyprocta Acouchy*, F. Cuv., having been placed on the table, Mr. Owen entered into a series of remarks explanatory of their peculiarities, which he pointed out with reference to the skeletons of other *Rodentia* exhibited for the purpose of comparison: the substance of Mr. Owen's remarks, together with a table presented by him of the admeasurements of the skeleton in both the above animals, have been printed in the "Proceedings" of the Committee.

The exhibition of the collection of Shells formed by Mr. Cuming on the western coast of South America and in the South Pacific Ocean, was resumed; and of these, the following new species were characterized by Mr. Broderip and Mr. G. B. Sowerby: viz. by the latter naturalist, CHITON *bipunctatus*, *exiguus*, (the smallest species Mr. Sowerby has seen, length $\frac{3}{18}$ths, breadth $\frac{1}{18}$th of an inch,) *catenulatus*, *graniferus*, *stramineus*, and *Pusio*; MARGINELLA *curta*; and by Mr. Broderip, BULINUS *Vexillum*, *pustulosus*, *Pupiformis*, *Panamensis*, *albicans*, *affinis*, *modestus*, *scutulatus*, *turritus*, *pulchellus*, *erosus*, *derelictus*, *varians*, *Tigris*, *Proteus*, (four well marked varieties of this species, forming a series of which the terms pass into each other by a gradual transition,) *mutabilis*, and *versicolor*.

May 22.—Mr. Yarrell exhibited skeletons and stuffed specimens of the *oared Shrew*, *Sorex remifer*, Geoff., which he had recently ascertained to be an inhabitant of Britain, and a species of *Arvicola*, also British, and apparently new to science: he characterized the latter as *Arv. riparia*, also modifying the characters of *Arv. agrestis*, on account of the near relation to it of the new animal, pointing out on the specimens exhibited the external and the osteological differ-

ences between these species, stating certain differences in their internal anatomy, and noticing those of their habits.

An extract was read from the '*Analyse des Travaux de la Société d'Histoire Naturelle de l'Ile Maurice, pendant la 2de Année:*' it was communicated to the Committee by its author, M. Julien Desjardins, Corr. Memb. Z. S., the Secretary of the Society whose labours are enumerated in it.

Among the novelties which have occupied that Society during the season of 1830–1831 have been some observations by M. J. Desjardins on the Zoology of the Mauritius as compared with that of the Isle of Bourbon, from which has resulted the curious fact, that notwithstanding that these islands are situated in such close proximity to each other, are of the same formation, and present a most remarkable analogy in their soil, their animals are not universally the same, some species being met with in one which never occur in the other.

In some remarks on the bones of the *Dodo*, (consisting of a *sternum*, a *cranium*, and four bones of the extremities,) which were sent by M. Desjardins to Paris, and which excited so much attention during the past summer from M. Cuvier and M. de Blainville, occasion is taken to correct some errors which have crept into the published statements respecting them. They were discovered, in 1786, in a cavern on the island of Rodriguez.

Mr. Gray exhibited living specimens of the *common Lizard, Lacerta agilis*, Linn., for the purpose of pointing out the marks of distinction between the sexes. The male is generally larger than the female, and more distinctly coloured; the under side of his body and base of his tail are very bright orange, while in the female these parts are pale yellowish green; his ante-anal scale is short and transverse, that of the female being much longer and hexagonal; and the under side of the base of his tail is flat, with a slight longitudinal middle depression just behind the vent, this part of the tail being in the female rounded and convex. In April and May the male may also be known by the base of the tail being dilated on the sides, just behind the thigh, a dilatation probably caused by the size of the *penes*, which are retracted into these parts.

Mr. Gray further explained various particulars of the habits of this species, observed by him in individuals which he had kept in a living state; and added, that in the only instance in which he had observed the *coitus*, one alone of the *penes* was inserted.

June 12.—The exhibition was resumed of the new species of *Shells* collected by Mr. Cuming on the western coast of South America and among the islands of the South Pacific Ocean.

The whole of the new species, thirty-nine in number, of the Genus Columbella contained in the collection, were illustrated by Mr. G. B. Sowerby. They are as follows: Columbella *pulcherrima, Harpiformis, bicanalifera, spurca, Buccinoides, coronata, lyrata, uncinata, elegans, unifasciata, gibberula, turrita, fulva, rugosa, fluctuata, recurva, lanceolata, maculosa, hæmastoma, varia, scalarina, pyrostoma* (This species somewhat resembles *Col. mendicaria.* Mr. Sowerby is

doubtful as to the propriety of admitting it among the *Columbellæ;* although wherever *Col. mendicaria* is placed this species must of course follow. Perhaps it might not be inconvenient to separate these from *Columbella*, and to combine them with their cognate species from among Lamarck's *Purpuræ*, *Ricinulæ* and *Murices*, and thus bring together a number of shells which would form a very natural genus.) *Maura* (somewhat related to the last, though partaking rather less completely of the characters of *Columbella*), *livida* (intimately related to the last two, forming a part of the same division of the genus), *nigro-punctata*, *obtusa*, *fuscata*, *costellata*, *guttata*, *varians*, *angularis*, *castanea*, *sulcosa*, *major*, *procera* (remarkable for its gigantic size, length $2\frac{3}{10}$, breadth $\frac{9}{10}$ inches), *pygmæa*, *unicolor*, *versicolor*, and *dorsata*. The characters of these species, together with those of all Mr. Cuming's shells exhibited at various meetings of the Committee, will be found in its "Proceedings."

June 26.—Specimens preserved in spirit were exhibited of two species of *Mus* collected by Lieut. Col. Sykes in Dukhun, both of which were apparently new to science. One of them is that referred to in Col. Sykes's 'Catalogue of the Mammalia noticed in Dukhun.' (Phil. Mag. and Annals, N.S., vol. x. p. 308.) It was characterized by Mr. Bennett as *Mus oleraceus*. The extreme length of the tail, which measures $4\frac{1}{4}$ inches, as compared with that of the body, which, including the head, measures only $2\frac{3}{4}$ inches, and the comparative length of the hinder tarsus, furnish characters sufficient to distinguish this *Indian field Mouse* from all its congeners. The second species belongs to that section of the genus *Mus* in which spines are intermixed with the fur. It was designated *Mus platythrix*.

Several imperfect skins of *Mammalia*, recently obtained by Mr. Gould from Algoa Bay, were exhibited; and Mr. Bennett remarked, that notwithstanding their deficiency in the most important particulars, they were yet of sufficient interest to claim the attention of the Committee, on account of the extreme rarity of two of the species to which they belonged, and of the probability that a third was altogether unknown to science.

One of them, the skin of a *Monkey* deficient as to head and hands, was, Mr. Bennett stated, evidently referable to the *Colobus polycomus*, Illig.; the long milk-white tail, strongly contrasting with the bright deep black fur of the body, being fully sufficient to characterize it. On the upper part of the skin, above the shoulders, some nearly white hairs were intermingled with the black ones. The only discrepancy observable between the specimen and the description of the species given by Pennant, was in the great length of the hairs of the body, the greater number of them being four or five inches long: this, it was remarked, might be dependent on age or locality.

Another skin, equally imperfect with the preceding, was that of the *Colobus ferrugineus*, Illig., with the state of which, described by M. Kuhl under the name of *Col. Temminckii*, the specimen agreed in every respect except in the absence of any yellow tinge in the rufous fur covering the under surface of the body.

The third skin was still more imperfect than the others, having

attached to it no portion of the neck, extremities, or tail, and consisting only of that of the body. Its length is 2 feet, its width $1\frac{1}{2}$. The dorsal portion is of a bright rufous fawn, which is continued on the shoulders and on the buttocks, but from which the red nearly disappears on the under surface, that being pale fawn. Across the whole of the back, commencing between the shoulders and passing backwards, a series of broad transverse glossy black stripes are seen, which run down the sides, becoming narrower towards the belly. These stripes are twelve in number, and are preceded and succeeded by a few similar, closer set, and fainter stripes, of a deeper rufous than the ground. The broadest of the dark stripes are on the loins, where they are fully an inch in width: their direction in passing down the sides is rather backwards. The commencement of a dark streak is also seen on the skin leading to the outside of the thighs. The quality of the fur is rather rigid, and the hairs are adpressed, resembling in these particulars the covering of the *Zebras*. It may not improbably belong to some species of *Antelope*, with which Europeans are yet unacquainted, but for which travellers to the country from whence the specimen was obtained may be induced to inquire, on being made aware of the existence of so beautiful an animal in that locality. The dark cross markings which ornament the fur are so uncommon among the *Mammalia*, that they alone will probably furnish a sufficient character to distinguish the quadruped in question from any other species inhabiting the interior of Africa, in the neighbourhood of Algoa Bay.

Several specimens were also exhibited of imperfect skins of *Cercopithecus Diana*, obtained from the same locality.

Specimens were exhibited of two species of *Hedgehog* from the Himalayan Mountains, which had recently been added to the Society's collection. Both of them belonged to that extra-European form of the genus *Erinaceus*, which is distinguished by the possession of long ears. The first was characterized by Mr. Bennett as *Erinaceus Spatangus*. The small size of this species (the total length of which is only $3\frac{1}{2}$ inches), its elongated form, the regular disposition of its spines, the more rounded form of its ears, and the comparative length of its hinder foot, distinguish it from the other species exhibited, which Mr. Gray was disposed to consider as the *Er. collaris* figured in the 'Illustrations of Indian Zoology,' but which Mr. Bennett rather regarded as a new species, it being destitute of a white collar, and differing in other particulars from the figure referred to. Mr. Bennett accordingly characterized it as the *Erinaceus Grayi*.

The exhibition was resumed of the new species of *Shells* collected by Mr. Cuming on the western coast of South America and in the islands of the South Pacific Ocean. Those exhibited on the present occasion were accompanied by descriptions from the pen of Mr. Broderip. They were as follows: BULINUS *rubellus*, and *Nux*; PARTULA *rosea*, *auriculata*, and *varia*; PLANORBIS *Peruvianus*; *Purpura muricata*; PECTUNCULUS *maculatus*, *ovatus*, and *intermedius*.

At the request of the Chairman Mr. Spooner read his notes of the *post-mortem* examination of the *Dromedary, Camelus Dromedarius*, Linn., which lately died at the Society's Gardens. The liver, kidneys, lungs, and heart, were greatly diseased.

July 12.—At the request of the Chairman, Mr. Arthur Strickland, of Boynton near Burlington, Yorkshire, exhibited a specimen, from his collection, of a *Puffin*, shot in the middle of August 1828, in a very stormy day, at the mouth of the Tees, which was apparently referable to the *Puffinus fuliginosus* (*Procellaria* (*Nectris*) *fuliginosus*, Kuhl). The *Proc. fuliginosa* of Solander's MS., though similar in size and colour, is entirely different, and at once distinguishable by having the bill short and powerful, and the nostrils in a raised tube, like the true *Procellariæ*. The *Proc. fuliginosa*, Lath., is also altogether distinct, being the *Thalassidroma Leachii*, Vigors: and the only description in the 'General History of Birds' which at all resembles the present species, is that of the *Proc. grisea*, a species distinct from that described under the same name by Linnæus. In its distinct and very little raised nostrils, the bird in question agrees with the *Shearwater Petrel, Puffinus Anglorum*, Ray: it has no back toe, but in lieu thereof a strong claw; and its tail is rounded. After characterizing this bird as *Puffinus fuliginosus*, Mr. Strickland concluded, by remarking, that although a single and perhaps purely accidental instance of a species appearing in this country may not fully entitle it to be ranked as a British bird, yet that the circumstance is worthy of being noticed, as it is only by carefully recording such instances as do occur, that we can decide what is entitled to that appellation, and be thereby enabled to perfect our local catalogues.

At the request of the Chairman, Mr. Gould exhibited numerous specimens of two *Birds* hitherto confounded under the name of *Motacilla flava*. In a communication which accompanied his exhibition, Mr. Gould explained the differences between the species, and entered at some length into their history. One of them, the *yellow Wagtail* of England, was described by Ray under the name of *Mot. flava*: its head is of a fine olive colour, and the stripe above and below the eye is of a bright yellow. The other, the *Mot. flava* of Linnæus, has the head of a lead colour approaching to blue, and the stripe above and below the eye of a clear white. The latter bird does not appear to have been ever met with in England: it is the one described by continental authors under the Linnæan name; while British writers have as constantly described under that name the bird to which it was originally given by Ray, and which regularly visits their own country. For Ray's bird, Mr. Gould suggested that the name of *Mot. flava*, under which it was described by our illustrious countryman, ought, according to the established rules of nomenclature, to be retained. To that of Linnæus, M. Temminck, and other continental authors, he proposed to apply the name of *Mot. neglecta*.

Mr. Owen referred to his Notes (published in the First Part of the 'Proceedings,' pp. 141 and 154, Phil. Mag. and Annals, N.S. vol. xi. p. 65 and 137) on the anatomy of individuals of two subgenera of the Linnæan genus *Dasypus*; one of which, the *Das. 6-cinctus*, Linn.,

had not, he believed, been previously dissected. He stated, that two other individuals of that species, one an adult female, the other a young one of the same sex, having subsequently come under his examination, he was enabled to confirm some of the peculiarities observed in the dissection of the young male specimen, and particularly the existence of the double *cæcum*, and the additional lobe of the lungs. He was also enabled to add to that account a description of the genital and mammary organs. In the absence of distinction between the *uterus* and *vagina*, in both species and in the mode of communication of what may be considered a single elongated uterine tube with the genito-urinary canal, may be observed the first traces of that approximation to the oviparous type of the genital organs which peculiarly characterizes the *Marsupial Edentata*.

Mr. Owen subsequently adverted to several external peculiarities which he had observed in the 6–*banded Armadillo*, and which, he remarked, were of some insterest, as connected with the burrowing habits of the animal. On the second toe from the inside there is a soft large cushion, evidently a modification of the organ of touch: at the hinder part of the fore-foot there is also a warty prominence, from which many hairs grow. There is a loose portion of integument below each eye, supported upon a prominence of the *zygoma*, hirsute, and resembling an inferior eyebrow; by means of which, and the coronal plate of armour above, the eye is well defended during the act of burrowing.

LINNÆAN SOCIETY.

Nov. 6.—Read an extract from a letter, addressed to Robert Brown, Esq., V.P.L.S. &c., by John B. Batka, apothecary and druggist at Prague, containing remarks on *Vateria indica* and some other plants. The *Elæocarpus copalliferus* of Retzius, which Kœnig and Vahl have referred to *Vateria indica* of Linnæus, M. Batka considers quite different. This plant was supposed to yield the copal; but M. Batka has examined specimens of its resin, as also of the resin of the true *Vateria indica*, which he finds different from each other, and also from copal, which is now ascertained to be the produce of *Hymenæa verrucosa*. The author expresses the difficulties he has met with in tracing the synonyma of *Laurus Cinnamomum* and *Laur. Cassia*, which after a careful examination he is inclined to consider identical; the plant which is found commonly cultivated in collections for *L. Cassia*, being *L. Malabathrum*, distinguished by its large, glossy, coriaceous leaves, thinner inflorescence, divided calyx, and smaller fruit. The differences observable in the specimens of these plants he attributes to the influence of soil and climate. The *L. javanensis*, which yields the bark brought from Java under the denomination of *Cassia lignea vera*, the author also regards as only a variety of *L. Cinnamomum*. The Chinese Cassia lignea bark, and the Cassia buds of commerce, M. Batka regards as the produce of an unknown species of the genus, distinct from the *Laurus dulcis* of Roxburgh, and perhaps identical with a species in Mr. Lambert's herbarium from Cavanilles, marked *L. manillensis*.

The Guinea grains and Madagascar cardamoms M. Batka considers as the productions of two different plants, the former of *Alpinia Granum Paradisi* of Afzelius, and the latter of *Alpinia madagascariensis*; both different from the *Alpinia melegneta* of Roscoe.

LXXXIII. *Intelligence and Miscellaneous Articles.*

ON CERTAIN POINTS, HITHERTO UNEXPLAINED, IN THE NATURAL HISTORY OF THE PAPUANS, OR ASIATIC NEGROS.

THE following sketch of the history of the Asiatic Negros, a race of people, who, of late years, have attracted much attention from naturalists and historians, was drawn up in the year 1828, for the purpose of being annexed to a biographical Memoir of the late lamented Sir Thomas Stamford Raffles, which had been commenced, in the preceding year, in the third volume of the Zoological Journal. That Memoir having been subsequently discontinued, the subjoined article has hitherto remained, unpublished, in the portfolio of the writer. It is now made public in the Philosophical Magazine and Journal of Science, as contributing, in some degree, to fill up a chasm in our knowledge of a very remarkable and interesting variety of the human species; and as offering a view of their ancient location in India and subsequent distribution over the regions south of that country, which, so far as the author's reading has extended, has never before been distinctly promulged, and certainly not in that generality which a careful investigation of the subject appeared to authorize him in giving to it.

It may be proper to mention, that since the completion of the inquiry, the results of which are summarily given in the following sketch, the attention of the author has been almost entirely withdrawn from subjects of this kind: he is not aware, therefore, whether the conclusions at which he had then arrived, have been in any degree either impugned or confirmed by subsequent discoveries; his own knowledge of the subject, at the present time, remaining just what it was when the sketch was originally drawn up.

The remarks on the existence of a woolly-haired race in south-eastern Asia, which are promised in a note appended to a passage in Sir S. Raffles's Discourse on the Sûnda Isles, &c., quoted in the Memoir before alluded to (Zool. Journ. vol. iii. p. 47), but the publication of which, in fulfilment of that promise, was prevented by the discontinuance of the Memoir, it may be well to add, are comprised in the sketch now given.

Oct. 4, 1832.

When the late Sir T. Stamford (then Mr.) Raffles returned to Europe, after having resigned the government of Java, in 1816, he was accompanied by a young Papuan, or native of New Guinea, whom, in the preceding year, he had rescued from slavery on the island of Bali. On his arrival in England, much curiosity was excited by the young Asiatic, who was the first individual of the

woolly-haired race of Eastern Asia that had ever been seen in this country. He was examined by the late Sir Everard Home, who drew up some remarks on his physical conformation, as compared with that of the African Negro race, which were published by Sir Stamford Raffles in the Appendix to his History of Java (edit. 1817, vol. ii. App. p. ccxxxv.), illustrated with an engraving of the Papuan, from a portrait by Phillips.

At the period of the discourse on the Sûnda Isles and on Japan, which he delivered before the Batavian Society of Arts and Sciences, in 1815, Sir Stamford appears to have been of opinion, that this woolly-haired race of modern Asia had been originally derived from the African continent, and that their existence throughout the Indian Archipelago, indicated, that extensive intercourse had taken place, in ancient times, between the Asiatic islands and Ethiopia. But the researches into the physical and political history of the varied population of the Indian Archipelago, into which he was led, subsequently, by the preparation for the press of the History of Java, which he published in 1817, appear to have shaken his belief on this point. For, in that work (*ubi sup.*), after noticing the opinion of their origin which he had formerly adopted, as just stated, and also the opposite notion of their being the aboriginal inhabitants of the countries through which they are now scattered, he merely says, "I shall content myself with observing, that they appear at the present day to form the bulk of the population of Papua or New Guinea*."

Since the publication of the History of Java, the scattered information which was previously extant respecting the people of the various Archipelagos in the Indian and Pacific oceans, has been collected together and combined; chiefly by those naturalists whose attention has been more especially devoted to the physical history of the human species: by this means, considerable light has been thrown upon the interesting subject now before us.

* Subsequently to the preparation of the above article for the press, the writer has observed another statement of Sir Stamford Raffles's views respecting the origin of the Papuan race, which it would be unfair to omit while discussing the fluctuation of his opinions on this subject. It is contained in a letter from Sir Stamford to Mr. Wilberforce, dated September 1819 (Memoir of the Life and Public Services of Sir T. S. Raffles, by his Widow, p. 410), and is as follows: "I am far from concurring in the opinion regarding the aborigines of these islands, and rather consider the Caffres [the term *Caffres* is used by Sir Stamford, throughout his correspondence, in reference to inhabitants of the Indian Archipelago, as synonymous with that of *Papuans*, as above defined] we now find in them to have been brought by traders in remote periods as slaves—as such they are generally considered and treated whenever entrapped." When taken in conjunction with Sir Stamford's former opinions, as cited above, this merely shows that his mind continued in a state of indecision on the subject, but inclining to a belief in the alleged African origin of the Papuans: being, however, nothing more than an opinion, it leaves the subsequent representations in the text untouched. Nothing farther occurs on the subject in the Memoir here quoted.

From a careful examination of the knowledge respecting the Asiatic Negros which has thus been obtained, and more particularly from their characters and history, as detailed in the works of Professor Blumenbach, Mr. Lawrence, Dr. Prichard, and M. Virey, and compared with the results of some of M. Julius Klaproth's profound researches into the ancient history of Asia, the present writer has been led to deduce the following view, of the original location and condition in India, and of the eventual dispersion, throughout the regions to the south and south-east of the eastern part of the Asiatic continent, of this singular race of mankind.

The actual inhabitants of Papua, or New Guinea, may be considered as constituting the type, at the present day, of the Papuan or Asiatic Negro race, properly so called. This race may be defined as the black savages with *woolly hair*, who are to be met with, from the interior of Malacca and Siam, and the islands in the gulf of Bengal, on the north, to Van Diemen's Land on the south. The facts and statements relating to the Papuan race, which are furnished by the authorities mentioned above, taken collectively, and mutually explained, and corrected or modified by each other, appear to lead to the following conclusions: First, that the Papuans have not been derived from the population of Africa, even if they should ever prove to be descended, as Negros, from the same original stock: Secondly, that (in conjunction perhaps, as will presently be noticed, with the black savages having *straight hair*, who are at present nearly co-extensive with them) they were the *Aborigines*, or at least the most ancient people of whom any traces can now be discovered, of both the great Peninsulas of India,—that is, of Hindûstan and Malacca, and probably also of the seat of the present Birman empire, Siam and Cochin China. And further, that the bulk of this race migrated, either voluntarily or compulsorily, from the Peninsulas, to the islands of the Indian Archipelago; whence again they extended, in process of time, through New Guinea (which at present constitutes as it were their *Metropolis*) to the Australian regions.

These Negros of Asia appear to have been driven from the Peninsulas as well as the great islands of India, or compelled to inhabit the interior fastnesses only of those countries, partly by the increasing ascendancy, and partly by the actual prowess, of some of the Caucasian and Mongolian races, who, whether or not they professed the Hindû or Bûddhaic faith, at the time when they subdued India and the "Farther East," were unquestionably, as settled in those countries, the lineal though remote ancestors of the present Hindûs and worshipers of Bûddha. The latter races of people, as is well known,—to a great extent on the continent of India, and almost wholly in the islands,—were afterwards subjugated, and, in some of the islands, annihilated, as a distinct people, by the Mohammedans.

Such also, it is probable, was the course of things with the black savages having *straight hair*, who exist nearly co-extensively with the Papuans, and by whom Australia itself is peopled; so that the Aborigines of India would appear to have consisted of these two

black races of mankind. The wide extent upon the globe of a people having the same physical peculiarities, is indicated, in a remarkable manner, by the fact, that the characters described by Sir Everard Home, as existing in the Papuan of New Guinea brought to England by Sir Stamford Raffles, are identical with those assigned to the inhabitants of Van Diemen's Land, so far distant to the south of New Guinea, by M. Peron and other naturalists.

This view of the subject, if followed out into particulars, would probably remove many of the difficulties and contradictions in which the history of the black people of India and the southern Archipelagos, is at present involved. There is one circumstance, however, in the natural history of these people, and that one of equal difficulty and interest, which, it must be acknowledged, would still remain unexplained: viz. The strong resemblance, as well mental as physical, which the lowest of the Papuan race, in intelligence, and in the scale of humanity in general (the natives of Van Diemen's Land, for example), bear to the *Hottentots* of Southern Africa; notwithstanding, that, as stated above, the entire history of the Papuan race is inimical to the supposition of its being derived from Africa;—notwithstanding the facts (forming part of that history) that although the extreme *aberrant* varieties of this group of mankind, and of that formed by the African Negro race (applying to the natural history of man some of the terms introduced into philosophic zoology by Mr. W. S. Macleay) are thus similar in character,—the intermediate varieties, as well as the normal or sub-typical Papuans of New Guinea and Negros of equatorial Africa, are manifestly distinct in character, being (as inhabitants of those countries, at least,) descended from different stocks. But this circumstance, apparently so anomalous, may be explained, probably, by the consideration, that, in these two sets of people, forming the zero points, as it were, of their respective races, we behold the human species, though derived from different stocks, and thus deviating from the type by different routes, in *parallel* extreme states of degradation, —*equally* distant, that is, from the type of the species, or the summit of the scale of human perfection.

The principal facts from which the foregoing deductions have been made, will be found in the following works: Lawrence's Lectures on Physiology, &c. First Edit. p 568, &c.; Prichard's Researches into the Physical History of Mankind, vol. i.; Virey, *Hist. Nat. du Genre Humain*, 1824, tom. i. p. 499. tom. ii. p. 16; and Julius Klaproth's Essay on the Authority of the Asiatic Historians, as translated in the Asiat. Journ. vol. xvi. p. 216, *et seq*.

E. W. B.

QUESTIONS AS TO THE CONTINUATION OF METALLIFEROUS VEINS FROM PRIMARY INTO SECONDARY FORMATIONS. BY A CORRESPONDENT.

To the Editors of the Phil. Mag. and Journ. of Science.

Gentlemen,

In Cornwall, the same veins, without interruption, traverse both

the granite and slates; their contents, however, varying in the different rocks.

Being myself confined by circumstances to a district of primary rocks, I beg permission, through the pages of your Journal, to inquire whether any of your geological readers have ever traced *the same vein* from a *primary* into a *secondary* rock? If so, what were the attendant phænomena? Were the *contents identical* in *both formations*? Or do the veins which traverse each of these different formations, respectively terminate when falling in contact with rocks of the other series?

In the present imperfect state of our knowledge of the phænomena of metalliferous veins, any information connected with the subject of this inquiry cannot fail to be highly interesting; and it strikes me, that the intelligent agents of the lead mines in North Wales might supply some valuable matter. I remain, &c.

Tavistock, September 19th, 1832. W. J. H.

NOTICE OF A NEW OXY-HYDROGEN BLOWPIPE APPARATUS. BY J. O. N. RUTTER.

I have caused to be constructed by Messrs. W. and S. Jones, 30, Holborn, an apparatus which is more simple, and at the same time more effective than either Clarke's or Gurney's blowpipe; and it possesses the additional advantage of being *perfectly safe*. The most timid may use this instrument without the slightest danger of explosion. With ordinary precautions such an occurrence is absolutely impossible.

In Clarke's and Gurney's blowpipes it is well known that the gases are mixed in their due proportions previously to changing the respective reservoirs. In this consists their principal cause of insecurity,—to obviate which I condense the gases in separate vessels, and they are not mixed until in a state of combustion.

Excepting that the vessels I employ are larger than ordinary, I may describe my apparatus as consisting of two of Clarke's blowpipes, fixed parallel to each other on a mahogany slab, the jets being inclined so as to form an angle of about 5°, and separated by a partition $\frac{1}{50}$th of an inch thick. The orifices of the jets are considerably larger than those commonly used.

The dimensions of the vessels are as follows:—That for hydrogen (marked HYD.), 10 inches long by 5 wide, and 4 deep; that for oxygen (marked OXY.), of half the capacity of the former, viz. 10 inches long, $2\frac{1}{2}$ wide, and 4 deep. It is important that the copper vessels be made very strong: this is the greatest difficulty I have had to contend with. With a 9-inch syringe I can condense from 800 to 1000 cubic inches of hydrogen gas into the largest vessel, and about half that quantity of oxygen into the vessel appropriated for it. That there can be no necessity for safety-valves, safety-tubes, wire-gauze, water, or oil, or mercurial chambers, must be apparent to every one whom the present communication may concern: these are consequently dispensed with. The tubes which conduct the gas from the respective vessels have each two stop-cocks to

regulate the escape. A very little practice enables the operator to determine the quantity so as to produce the maximum of heat.

The usual experiments as performed by the apparatus I have thus, I fear, imperfectly described, are, if I may be allowed the use of the expression, *infinitely* more splendid and more impressive than can be effected by any other means with which I am acquainted. The lime experiment, especially, is inconceivably brilliant, exhibiting a disc of pure white light $1\frac{1}{2}$ inch in diameter. With a piece of clock-spring I have filled an area of 3 feet diameter with the most beautiful coruscations.

The advantage of this apparatus is that of sufficient capacity that one or two charges will be sufficient for a course of illustrative experiments in a lecture-room. There is not the slightest danger of explosion. It is more powerful and more striking in its effects than any other instrument.

I shall have great pleasure in furnishing any further details that may be required.—Might not vessels of sufficient strength and capacity be constructed in which a store of gas could be kept at the most important light-houses, to be used in thick weather, in furtherance of Lieut. Drummond's plan?

Dr. Faraday has informed me that about the time that Clarke's blowpipe was invented, an instrument somewhat similar to mine was shown him, and was, he believes, described in the Phil. Mag. But that instrument consisted of one vessel only, divided by a diaphragm. Hence there was no security against an explosive mixture forming in either of the chambers through a defect in the metal.

Lymington, Hants, Sept. 10, 1832.

NOTICE OF A MARINE DEPOSIT IN THE CLIFFS NEAR FALMOUTH. BY R. W. FOX.

Many persons are no doubt aware, that in some parts of the cliffs between Falmouth and Helford harbours, there exists an horizontal bed of rolled quartz pebbles, gravel and sand, similar in every respect to the materials which prevail on the contiguous sea-shore. Hence it cannot be questioned that the origin of both is the same; and I think it may also be assumed, from the above-mentioned materials being in many parts arranged in separate layers in the bed, that the sea must have frequently risen to its level.

The thickness of the bed varies from one to three feet and upwards, and it is situated generally about nine to twelve feet above the level of the highest spring tides. I have not yet extended my observations on this bed beyond about four miles of coast, but within these limits it seems almost everywhere to exist when the cliffs are not composed of solid rock. This bed does not appear to penetrate far into the cliff, if we may judge from the few parts where it has been broken away or cut through. In one place I have observed it about eight feet, and in another twenty, within the face of the cliff. The rocks on this coast are of clay-slate, having a very considerable underlie, mostly towards the S.E.; but the bed in question is found only in those parts of the cliff which are composed of earth, stones, and de-

tached rocks, and which rest upon the bed, and are also under it in many places. It seems, in fact, that these substances have fallen upon the bed, and covered it over after its formation by the sea; and in time the mass has become so consolidated, as often to present nearly a perpendicular surface to the sea, of from thirty to fifty or sixty feet in height.

In some parts of the cliff, particularly within the parishes of Budock and Mawnan, the pebbles and gravel have been formed into a conglomerate, apparently, by the oxides of iron and manganese. The bed may have been produced by a succession of extraordinarily high tides, resulting from some long operating or more temporary cause, at a very remote period. However this may have been, the fact is curious, and seems to be at variance with the notion, that the sea has made considerable encroachments on the coasts of Cornwall.

If it should be surmised that the land itself might have been elevated, it may be remarked, that such an hypothesis is not in accordance with the horizontal position of the bed for so considerable a distance, notwithstanding that the cliffs are in many places intersected by valleys.

INQUIRIES RESPECTING THE DIMENSIONS AND VALUE OF THE LOCAL MEASURES IN COMMON USE AT COVENT GARDEN MARKET. BY B. BEVAN, ESQ.

To the Editors of the Phil. Mag. and Journal of Science.

Sometime last year, I inquired through the medium of the Gardener's Magazine, (as the most probable channel for the information sought,) the dimensions of the local measures in common use at Covent Garden market; at present no one has thought proper to favour the public, through that channel, with a specification of those measures, which are generally unknown to country gardeners, and on that account the relative prices of fruit and vegetables in the country are also unknown.

The *Sieve*, being a measure frequently used, its diameter and depth should be specified.

The *Half-sieve* also,—for although it is so denominated, it may not perhaps usually contain half the quantity of a sieve.

The *Punnet* should also be defined, by specifying its dimensions, or by naming its proportion to some known measures.

The *Pottle*, is a measure already known; but probably there may be some variation in the local pottle of Covent Garden and the Imperial pottle.

There are some other articles sold by the *Bunch*, which to a countryman is an undefined quantity. If any person would take the trouble to ascertain the weight of these bunches, and favour the public, through the medium of your Magazine, with the information, it would be acceptable to many in the country.

The trouble of ascertaining the dimensions of the measures above described, will be very little.

The gallon and pottle measures, perhaps, may be not quite conformable to the general Act, in the proportion of their diameters to their depths. Yours, &c. B. BEVAN.

AN EPHEMERIS OF THE STARS PROPER TO BE OBSERVED WITH *MARS*, AT THE PRESENT OPPOSITION OF THAT PLANET.

[Concluded from page 407.]

1832.	Stars.	Mag.	Apparent Place. Right Ascens.			Apparent Place. Declin. *North.*			Semidiameter. In time.	Semidiameter. In arc.	Hor. Par.
			h	m	s	°	′	″	s	″	″
Dec. 7	65 Arietis	6	3	14	48,30	20	12	19,7			
	Mars[1]	N		19	58,38	20	5	29,0	0·601	8,46	15,15
	F[1] Tauri	6·7		32	41,48	19	9	32,1			
8	65 Arietis	6	3	14	48,30	20	12	19,7			
	Mars[1]	S		19	1,62	20	3	48,7	·596	8,39	15,02
	F[1] Tauri	6·7		32	41,48	19	9	32,1			
9	65 Arietis	6	3	14	48,31	20	12	19,8			
	Mars[1]	N		18	8,11	20	2	15,4	·591	8,32	14,89
	F[1] Tauri	6·7		32	41,48	19	9	32,1			
10	65 Arietis	6	3	14	48,31	20	12	19,8			
	Mars[1]	S		17	17,89	20	0	49,5	·586	8,25	14,76
	F[1] Tauri	6·7		32	41,48	19	9	32,1			
11	(38) Arietis	8	3	11	15,80	19	54	0,6			
	65 ——	6		14	48,31	20	12	19,8			
	Mars[1]	N		16	31,00	19	59	31,3	·581	8,18	14,63
12	(38) Arietis	8	3	11	15,80	19	54	0,6			
	65 ——	6		14	48,31	20	12	19,8			
	Mars[1]	S		15	47,47	19	58	21,2	·575	8,10	14,49
13	(38) Arietis	8	3	11	15,80	19	54	0,6			
	65 ——	6		14	48,30	20	12	19,8			
	Mars[1]	N		15	7,33	19	57	19,5	·570	8,02	14,36
14	(38) Arietis	8	3	11	15,80	19	54	0,7			
	Mars[1]	S		14	30,62	19	56	26,4	·564	7,94	14,22
	65 Arietis	6		14	48,30	20	12	19,8			
15	(38) Arietis	8	3	11	15,80	19	54	0,7			
	Mars[1]	N		13	57,33	19	55	42,1	·558	7,87	14,09
	65 Arietis	6		14	48,30	20	12	19,8			
16	(38) Arietis	8	3	11	15,80	19	54	0,7			
	Mars[1]	S		13	27,49	19	55	6,9	·553	7,79	13,95
	65 Arietis	6		14	48,30	20	12	19,8			
17	(38) Arietis	8	3	11	15,79	19	54	0,7			
	Mars[1]	N		13	1,10	19	54	41,1	·548	7,72	13,81
	65 Arietis	6		14	48,30	20	12	19,8			
18	(38) Arietis	8	3	11	15,79	19	54	0,7			
	Mars[1]	S		12	38,15	19	54	24,6	·542	7,64	13,67
	65 Arietis	6		14	48,30	20	12	19,8			
19	(38) Arietis	8	3	11	15,79	19	54	0,7			
	Mars[1]	N		12	18,64	19	54	17,8	·536	7,56	13,54
	65 Arietis	6		14	48,29	20	12	19,8			

1832.	Stars.	Mag.	Apparent Place. Right Ascens.	Apparent Place. Declin. *North.*	Semidiameter. In time.	Semidiameter. In arc.	Hor. Par.
			h m s	° ′ ″	s	″	″
Dec. 20	(38) Arietis	8	3 11 15,78	19 54 0,7			
	Mars[1]	S	12 2,56	19 54 20,9	0·531	7,49	13,40
	65 Arietis	6	14 48,29	20 12 19,8			
21	(38) Arietis	8	3 11 15,78	19 54 0,7			
	Mars[1]	N	11 49,87	19 54 33,6	·526	7,41	13,26
	65 Arietis	6	14 48,29	20 12 19,8			
22	(38) Arietis	8	3 11 15,78	19 54 0,7			
	Mars[1]	S	11 40,61	19 54 56,4	·520	7,33	13,12
	65 Arietis	6	14 48,28	20 12 19,8			
23	(38) Arietis	8	3 11 15,77	19 54 0,7			
	Mars[1]	N	11 34,72	19 55 29,2	·515	7,25	12,99
	65 Arietis	6	14 48,28	20 12 19,8			
24	(38) Arietis	8	3 11 15,77	19 54 0,7			
	Mars[1]	S	11 32,18	19 56 11,9	·510	7,18	12,85
	65 Arietis	6	14 48,27	20 12 19,8			
25	(38) Arietis	8	3 11 15,76	19 54 0,7			
	Mars[1]	N	11 32,95	19 57 4,7	·505	7·10	12,71
	65 Arietis	6	14 48,27	20 12 19,8			

LUNAR OCCULTATIONS FOR DECEMBER.

Occultations of Planets and fixed Stars by the Moon, in December 1832. Computed for Greenwich, by THOMAS HENDERSON, *Esq.; and circulated by the Astronomical Society.*

1832.	Stars' Names.	Magnitude.	Ast. Soc. Cat. No.	Immersions. Sidereal time.	Immersions. Mean solar time.	Immersions. Angle from North Pole.	Immersions. Angle from Vertex.	Emersions. Sidereal time.	Emersions. Mean solar time.	Emersions. Angle from North Pole.	Emersions. Angle from Vertex.
				h m	h m	°	°	h m	h m	°	°
Dec. 6	63 Tauri*	6	490	7 37	14 33	24	61	7 50	14 46	0	38
7	104 *m* Tauri	5	592	23 3	5 57	79	39	23 52	6 46	313	272
8	15 Gemin.	6	799	7 16	14 5	52	71	8 12	15 0	308	337
10	δ Cancri*...	4·5	1066	12 25	19 5	357	37	12 39	19 19	331	11
17	ξ^1 Libræ ...	6	1688	10 7	16 20	100	64	11 0	17 13	210	178
18	η Libræ....	4·5	1787	10 0	16 9	82	43	10 58	17 7	234	199
27	74 *K* Aquar.	6	2732	2 39	8 14	111	144	3 45†	9 19	297	334

* These occultations doubtful—perhaps appulses only.
† Emersion in horizon.

Meteorological Observations made by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; by Mr. GIDDY at Penzance, and Mr. VEALL at Boston.*

Days of Month, 1832.	Barometer. London. Max.	London. Min.	Penzance. Max.	Penzance. Min.	Boston $8\frac{1}{2}$ A.M.	Thermometer. London. Max.	London. Min.	Penzance. Max.	Penzance. Min.	Bost. $8\frac{1}{2}$ A.M.	Wind. Lond.	Penz.	Bost.	Rain. Lond.	Penz.	Bost.
☽ Oct. 1	30·037	30·000			29·46	71	53			57	s.		w.	0·53		...
2	29·964	29·923			29·36	69	42			59	sw.		calm	·02		...
3	29·918	29·789			29·37	68	56			53·5	s.		w.	·06		...
4	29·769	29·579			29·11	63	52			58	sw.		calm	·20		...
5	29·416	29·231			28·82	61	42			55·5	s.		E.	·57		0·48
6	29·599	29·508			28·97	63	35			48	s.		NW.	·26		...
7	29·654	29·564			28·97	58	50			51	sw.		w.	·41		.30
8	29·708	29·166			28·63	56	43			53·5	sw.		SE.	·02		·24
○ 9	30·006	29·869			29·15	64	50			50	w.		w.	·07		·07
10	30·230	29·998			29·33	67	55			57	sw.		calm	...		·17
11	30·305	30·295			29·54	72	56			60	w.		w.	...		...
12	30·077	29·934			29·28	63	47			60·5	s.		w.	·12		...
13	30·175	29·938			29·18	62	37			52·5	sw.		w.	·02		·14
14	30·327	30·195			29·71	62	40			51	s.		calm	...		...
15	30·314	30·275			29·60	64	51			52	w.		calm	·04		...
☾ 16	30·320	30·189			29·48	60	35			54	w.		w.	...		·15
17	30·401	30·347			29·80	60	41			46	sw.		w.	...		...
18	30·312	30·261			29·66	60	50			53	sw.		calm	·08		...
19	30·302	30·248			29·70	55	30			47	NE.		NW.	·14		...
20	30·369	30·340			29·80	57	37			46·5	NE.		calm	...		...
21	30·375	30·318			29·85	56	31			45	NE.		calm	...		...
22	30·313	30·256			29·75	58	37			51	E.		calm	...		...
● 23	30·352	30·341			29·85	56	40			53·5	NE.		calm	...		...
24	30·436	30·366			29·90	51	41			53	E.		SE.	...		...
25	30·471	30·422			29·90	52	38			50	E.		calm	...		...
26	30·412	30·382			29·87	53	36			47	w.		calm	...		...
27	30·399	30·345			29·85	52	42			43·5	w.		calm	·11		...
28	30·271	30·142			29·70	55	43			49	s.		calm	·03		...
29	30·126	29·936			29 47	55	35			53	s.		s.	·17		...
30	30·199	30·113			29 62	56	41			45	sw.		calm	...		·30
☽ 31	30·124	29·949			29·56	61	47			45	sw.		NW.	·24		...
	30·471	29·166			29·49	72	3			51·6				3·09		1·85

Remarks.

London. — October 1. Slight fog: fine. 2. Rain: fine. 3. Foggy: rain. 4. Rain. 5. Heavy rain in the afternoon. 6. Fine. 7, 8. Stormy, with rain. 9. Fine: rain at night. 10—15. Fine. 16. Rain: fine. 17. Fine. 18. Foggy. 19. Rain. 20. Slight frost: very fine. 21—24. Dense fog in the mornings: fine. 25. Hazy. 26, 27. Foggy 28. Rain: foggy. 29. Rain: clear at night. 30. Fine: overcast. 31. Fine: rain at night.

Boston.—Oct. 1—4. Cloudy. 5. Cloudy: rain early A.M. 6. Fine. 7. Fine: rain early A.M. 8. Rain. 9. Stormy. 10. Cloudy: rain early A.M. 11. Fine. 12. Stormy. 13. Fine: rain early A.M. 14, 15. Fine. 16. Fine: rain early A.M. 17. Fine. 18. Cloudy. 19. Fine. 20, 21. Cloudy. 22. Fine. 23—26. Cloudy. 27. Misty. 28. Fine. 29. Rain. 30. Cloudy. 31. Fine.

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* Mr. GIDDY's Observations for this Month have not been received.

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END OF THE FIRST VOLUME.

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